

Aspects of the Social Organisation of 'Male Infertility'

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How do I know what I think till I see what I say?

(E.M. Forster, quoted in Sperling, 1990: 288)

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Abstract

This dissertation revolves around three main elements: 'male infertility'; existing social science research on infertility; and ethnomethodology. The substantive topic 'male infertility' is enclosed in quotation marks for two reasons. First, following the overall form of ethnomethodological inquiry, the aim is to explicate how the sense and order of 'male infertility' is constituted through available socially organised procedures; hence, the quotation marks are used to 'bracket' the phenomenon and focus on the methods that make it available. Second, 'male infertility' is a convenient shorthand topic label, a general organising concept, as opposed to a precise label for a tightly defined phenomenon.

While this study's approach makes it very different to existing sociological studies of infertility, the difference is not to the extent of isolation - a strong attempt is made to engage with prior studies. Often this engagement takes a critical form, the general argument being that sociological studies which approach phenomena for the way they 'bear the marks' of societal structures, will ignore the incarnate orderliness of social action - that is, the way social action is readily explicable to members, in and as it occurs, using the resources at-hand, with 'no time-out'. Ethnomethodology suggests that this ready explicability is based upon taken-for-granted, socially organised sense-assembly practices - thus, this study's argument that the content, the intelligibility of 'male infertility', is interdependent with the social scenes and embedded socially organised procedures, with and within which 'male infertility' is found. Form and content stand or fall together. Consistent with this viewpoint, four empirical analyses of the social organisation of 'male infertility' are offered. The specific

topics discussed are: the conversational disclosure of infertility; the language of reproduction; humour and infertility; and high rates of non-response by men in studies of infertility.

In general, the empirical analyses are 'indifferent' to the topic of study, that is, there is no overriding aim of offering practical correctives or broader socio-political critique. However, in at least one empirical chapter a more critical stand is taken, and, in the concluding chapter, it is argued that an ethnomethodological descriptivist approach can have socio-political implications. Overall, the study supports the growing trend for ethnomethodological insights to be utilised in the study of substantive topics; and, since the dissertation is a form of writing 'anew', it can be considered to minimally change 'male infertility' as a form of life.

Chapter One

Framing the Study: Inclusions and Exclusions

1.1 *Introduction*

In order to frame this study, it is useful to begin by considering not only what the study includes, but also what it excludes. While description of exclusion could go on indefinitely, in limited amounts it is a useful technique for framing a study. For example, contrast the chosen title - "Aspects of the Social Organisation of 'Male Infertility'" - with one that was rejected - "Male infertility: Men without issue?".¹ The chosen title seems nondescript in comparison to the 'catchiness' of the rejected title, which has the added bonus of providing a useful problem for sociological study: do 'men without issue' really find their infertility an 'issue'? Given the utility of the rejected title, it is natural to ask why it was rejected - why was a much more nondescript title chosen instead? The short answer is that the rejected title embodies the approach of studying male infertility as an 'issue', a social problem, and such an approach is not the principle aim of this study. The aim is to bring an ethnomethodological perspective to the study of 'male infertility', and although the chosen title may seem opaque to the untrained, it is intended to resonate with ethnomethodological meaning.

¹ The term 'without issue' is an outdated way of referring to infertility, nonetheless still clear in its meaning.

Following from the example of the title, the introduction will continue discussing what this study includes and excludes in the belief that it is a useful means of detailing the overall nature of this ethnomethodological study of 'male infertility'. It is not used to suggest that what is included is necessarily superior to what is excluded, but to show the relation of this study to other sociological studies of infertility. Overall, the introductory chapter is organised as follows:

- 1.2 Unpacking the title;
- 1.3 'Social organisation' versus 'social construction': a clarification of the ethnomethodological position;
- 1.4 The missing literature review;
- 1.5 Data description;
- 1.6 Final introductory points developed from an observation about the data.

Before proceeding to section 1.2, it is worth stressing one overall characteristic of this study. In essence, the study is empirical rather than theoretical, and this helps to explain the contents of the introductory chapter, as well as the remainder of the dissertation. As can be seen from the outline above, this chapter includes discussion of 'existing literature' (1.4), 'theory' (1.3), and 'data and methods' (1.5) (although these specific headings are not used). These matters are traditionally, in dissertations and theses at least, given much more space in separate chapters. Hence, it might seem somewhat presumptuous or risky to attempt coverage of all three areas in a single introductory chapter.

However, this is where the empirical emphasis of the study can be usefully stressed: the aim is to produce original empirical analyses of 'male infertility' and not to pursue lengthy, abstract theorising. The upshot of this emphasis is that the empirical chapters form the core of the dissertation. Moreover, they are self-contained works which pursue specific sub-topics and do not depend upon arguments in other chapters for plausibility of argument. Certainly, ethnomethodology is used as an overarching framework, and there are common theoretical arguments made in the empirical chapters, but there is no explicit discussion of these features either here or in the concluding chapter. Rather, theory is embodied in the consideration of empirical materials, the aim being to keep theoretical arguments relevant to descriptions of real-world materials and the specific problems addressed in each chapter.

That said, it can be noted that this introductory chapter is not devoid of theoretical discussion. What is offered does not take the form of an overview of ethnomethodology - a familiarity with this tradition is assumed - but is more tightly focused on explaining what it means to study the social organisation of 'male infertility'.

1.2 *Unpacking the Title*

Perhaps the most notable feature of the title is the enclosure of 'male infertility' in quotation marks. An explanation of this, and the use of 'aspects', can be provided by first explaining the use of 'social

organisation'. Some subtleties in the use of this term will be noted in the next section, but for now it can be explained relatively simply.

The term 'social organisation' is a straightforward enough phrase which can, and does, appear within many forms of social scientific writing. However, it does seem to be used more frequently within ethnomethodology, where it is used with a particular meaning and purpose in mind. The question of who first started using the term is immaterial, but certainly Garfinkel peppered his foundational *Studies in Ethnomethodology* (1967) with talk of the "organized activities of everyday life" or "organizations of commonplace everyday activities".² With Garfinkel and many other influential ethnomethodologists using the term, 'organisation' quickly became a feature of ethnomethodological writing.

Again it hardly matters where, but somewhere in the ethnomethodological lineage the term 'social organisation' appeared and gained some favour alongside the established use of 'organisation'. For example, jumping twenty years from the publication of *Studies in Ethnomethodology*, we find Button and Lee entitling an important collection of ethnomethodological work, *Talk and Social Organisation* (1987).³ In this work 'social organisation'

² Pre-dating Garfinkel's talk of "organized activities" or "organizations of activities" is Bittner's classic paper (1965) "The concept of organization". In this paper Bittner addressed his comments to an established body of literature on "formal organisations", that is, "stable associations of persons engaged in concerted activities directed to the attainment of specific objectives" (1965: 239). Bittner argued for a reconceptualisation of the study of organisation, asking how members used 'organisation' as a device for solving a multitude of practical problems. Thus, the focus is on procedural rules governing language-in-use, rather than organisations as units of people working together, and in this sense his approach is similar to that of Garfinkel.

resonates with ethnomethodological meaning, as shown in the following passage:

... C.A. is concerned to study *the social organisation of natural language-in-use*. ... it seeks to find ways not only of revealing society and social organisation as an achievement but of observing that order and achievement from inside. That is, instead of using data as a resource to test theories as to the nature of social organisation, it examines the social organisation of materials in an attempt to describe and understand that nature. It seeks, therefore, to describe social organisation of natural interactions asking what their order looks like and how it is put together by those who, in fact, produce it. ... The concern is not to try and fulfill some theoretical version of what constitutes a proper 'science' of society but is instead to determine the orderly ways in which conversationalists shape their talk with one another in mind thereby constituting it as a socially organised phenomenon.
(Button & Lee, 1987: 2-3, original emphasis)

This passage is in effect a description of ethnomethodological investigation, and clearly 'social organisation' is centrally important to this description. In brief, we see a focus on the question of how everyday social scenes are rendered orderly by those engaged in social action - how are social scenes so readily explicable to members? A large part of the answer is that phenomena are what they are because they are embedded within socially organised practices, hence the importance of the term 'social organisation'.

More details will be provided on the use of 'social organisation' in section 1.3, where it will be contrasted with the apparently similar term 'social construction'. But the immediate question is, why the

³ This is actually a collection of conversation analytic studies, but as made clear in Button and Lee's preface, conversation analysis is a form of ethnomethodological inquiry. Despite the degree of its specialisation, most contemporary ethnomethodologists would have no difficulty with the continued inclusion of conversation analysis within ethnomethodology; indeed, Heritage (1984) has called it ethnomethodology's 'jewel in the crown'.

quotation marks around 'male infertility'? To a large degree the answer is tied up with an implication of the emphasis on 'social organisation'. The ethnomethodological emphasis on the way phenomena are socially organised, rather than on the phenomena themselves, resulted in the use of so called 'bracketing' devices ('bracketing' is particularly prevalent in early ethnomethodology, but not so common now). These devices were means "to topicalise the production (or *doing*) of activities formulated by expressions within the brackets" (Lynch, 1991: 79, original emphasis). Bracketing transforms the focus of inquiry from phenomena which may be presumed to exist in their own right, to the procedures whereby phenomena are constituted (some subtleties of this position will be noted in the next section). It is partly in this sense that 'male infertility' is enclosed in quotation marks: I wish to avoid treating 'male infertility' as a resource for either, the application of prior theory, or for inquiry into how external societal factors supposedly construct its form; instead, the emphasis is on asking how the sense and order of 'male infertility' is constituted through available socially organised procedures.

A correlate of the use of bracketing is to direct inquiry to the use, in actual social scenes, of the expression that is bracketed. Without elaborating on this step, it points to another reason for enclosing 'male infertility' in quotation marks, and additionally, it helps explain the use of 'aspects' in the title. To put it simply, the expression within the brackets - male infertility - is being used as an organising concept. It is a convenient conceptual term which is both short enough to be used as a topic label, and sufficiently accurate as a summary of what the

inquiries in this study are about. This point can be clarified by first considering a simplistic rendition of an ethnomethodological study of 'male infertility', and then a brief description of the four empirical chapters that are actually on offer in this study.

As any number of sociological studies note (eg. Becker & Nachtigall, 1992; Gerson, 1989; Sandelowski, 1990a; Scritchfield, 1989a; Strickler, 1992), infertility is an historically contingent phenomenon which only recently became firmly centred within the realm of medicine. As with all classificatory activities, there are semantic problems and issues about correct definition,⁴ but the medical meaning of 'male infertility' is clear enough: a couple are unable to conceive after the period of time in which most couples conceive, and all or part of this inability to conceive is traceable to the male partner. Thus, if we were to study the use of 'male infertility' in actual social scenes, one obvious thing we could be studying is the medical classification practices where the label 'male infertility' is used. Conceivably, this could involve an investigation into the social organisation of laboratory activities where semen is collected, analysed, and classified.

⁴ This is a large topic not pursued in this study, because, for one, there is sufficient debate on the matter already. The problems referred to include: naming practices - choosing between, infertility, infecundity, reproductive impairment, involuntary childlessness, or something else (eg. see Leridon, 1981; Matthews & Matthews, 1986b); standardisation - are different studies of infertility based on the same referent (eg. see Greil, Leitko & Porter, 1988); and medical classification - what should classification of infertility be based on (eg. see Menken, Trussell & Larsen, 1986; Marchbanks *et al.*, 1989). But more importantly, the issue is not pursued here because "adequating the real" (McHoul, 1982) is not part of this study.

There are several ethnomethodological investigations which could be used as general models for such an investigation (eg. Garfinkel, Lynch & Livingston, 1981; Lynch, 1985; Rawlings, 1989), and these studies do utilise, mostly implicitly, a bracketing procedure. For example, Garfinkel, Lynch and Livingston's study of the discovery of a pulsar is not concerned with the question of whether or not Cocke and Disney 'really' discovered a pulsar, or what the meaning of it is; instead they conceptualise the pulsar as a "cultural object" and focus on "the way that the object-in-hand participates in the organization of its discovery" (Peyrot, 1982: 269). Counting sperm and classifying their shape while looking down a microscope, obviously inverts the scale of discovering a pulsar looking up a telescope, but clearly semen analysis could be studied in the same manner (if one could get access to such sites). But the point to be taken here is that if one works with a more conceptual view of 'male infertility', then there are a large number of topics that can be investigated in an equally ethnomethodological manner. The simplest way to get this point across is to outline the empirical analyses that are actually presented in chapters two to five.

In chapter two the topic of inquiry is the conversational disclosure of infertility. Discussion begins with an outline and critique of two existing models of disclosure - Foucauldian confession and a dramaturgical model - after which extracts from interview data are used as a tentative description of conversational disclosure of 'male infertility'. Work from conversation analysis is used to point to a possible procedural infrastructure for infertility disclosure, with the general point made that an analysis of the endogenous orderliness of

conversation is essential to understand infertility disclosure - it is this emphasis that the other two models lack. Regarding the bracketing device, it should be noted that the analysis of the disclosure of 'male infertility', presumably, applies equally as well to 'female infertility'; and indeed the discussion uses the general phrase 'infertility disclosure'. Thus, although the data are extracts from interviews with infertile men, the discussion is framed in general terms, and where the term 'male infertility' is used it is mainly as an organising concept.

Chapter three begins by discussing some feminist research which claims that the language of reproduction displays a male bias which contributes to, and is reflective of, gender inequality in society. Problems with this view are identified, and to justify and elaborate the critique of the feminist arguments, an analysis of a medical journal editorial is presented. This editorial does not focus on 'male infertility', but is about the conviction of a doctor for various illegal practices committed while offering donor insemination services. Thus, clearly the chapter fits within 'male infertility' as an organising concept but not so clearly as a bracketing device.

Similarly, in chapter four 'male infertility' is considered in light of other discussions, in particular an argument by Michael Mulkay (1988) about the nature of humour in modern society. The chapter begins by noting the readiness of people to find humour in infertility, but then discussion moves to an outline and critique of Mulkay's argument, followed by an alternative ethnomethodological approach to humour. In this alternative approach empirical data of relevance to 'male infertility' are used, but again, the overall argument is a generic

one, the point being that humour can be analysed as a local production, seriously accomplished. Thus, chapter four has no sustained focus on 'male infertility', but on the other hand it is not as if 'male infertility' is irrelevant to the analysis. The aim is to achieve some kind of balance between inquiry into an interesting sociological topic and inquiry into the social organisation of 'male infertility'.

The final empirical chapter uses researchers' discourse on infertility as data for analysis. Specifically, researchers' accounts of the high rate of non-response by men in studies of infertility are analysed for the imbrication of commonsense in social science reasoning. Non-response is found to be readily explicable, usually not by reference to any 'evidence' provided by non-respondents, but by the commonsense procedure of inferring from what is already presumed to be well established about the nature of infertility. In this chapter the tendency is to speak about 'infertility' just as much as about 'male infertility'. Good reasons for this are apparent in the chapter, but again it can be noted that the chapter combines 'male infertility' as a general organising concept and 'male infertility' in the sense of ethnomethodological bracketing.

A brief reference to an example of conversation analytic work helps to clarify this important point. Consider Tsui's (1991) research paper on "The pragmatic functions of *I don't know*".⁵ Throughout the paper Tsui presents *I don't know* in italics, and although she provides no elaboration, two reasons can be assumed for this practice: first, it sets the utterance apart from the remainder of the text as the topic of

⁵ A point made in this paper is briefly referred to in chapter four.

discussion, that is, it marks it as talk taken from ordinary conversation; and second, there is a clear sense in which it follows the ethnomethodological convention of bracketing. That is, the concern is not with the referent of the utterance, but with the activities that the utterance accomplishes (ie. its "pragmatic functions"). Now this is the point to be taken: 'male infertility' can be a verbatim utterance within written or spoken discourse, but it is also understandable as a conceptual term that can be applied to many situations where it is not actually used. An example of the former is where a medical record card includes 'male infertility' as a diagnostic label; an example of the latter is where an infertile male discloses his infertility to a stranger in an ordinary conversation without using the specific term 'male infertility'. While the words 'male infertility' have not been uttered, it is perfectly reasonable to collect this second event, and others like it, within an analysis of the "social organisation of 'male infertility'".

Overall, it is the latter usage which predominates in this study, but both senses of 'male infertility' appear throughout the study and may overlap to some extent. While this may seem inexact, there is no need for an apology, as the following two points suggest. First, it is made clear in the introduction to each empirical chapter just what the specific topic of inquiry is. As noted above, the empirical chapters are stand-alone pieces - the chapters are not intended to build upon each other to ultimately produce a comprehensive view of 'male infertility'. This leads to the second point: it should now be recognised that attempts to provide comprehensive views of social phenomena will be ill-fated. As Silverman (1989) has put it, sociologists should now "recognise that the phenomenon always

escapes" - it is not possible to 'capture' an experience by ethnographic, or any other, methods. This is not an excuse for poorly setting out what it is one is studying. It is simply to recognise that "idealized conceptions of phenomena become like a will-o'-the-wisp on the basis of systematic field research, dissolving into sets of practice embedded in particular milieux" (Silverman, 1989: 221). Clearly, my aim in this research is to focus on the sets of practice, or procedures for sense-assembly, within which 'male infertility' is embedded, rather than on a ill-fated attempt to describe the essence of 'male infertility'.

Very briefly, the above points should also make it clear why 'aspects' appears in the title. This study offers inquiry into only selected aspects of the social organisation of 'male infertility'. Perhaps this is stating the obvious as any reader can assume selectivity, but in the absence of 'generic readers' the preference is to make this selectivity explicit.

A final matter to note here, is that from this point on the brackets around 'male infertility' will be dispensed with. There are good reasons for the use of brackets around 'male infertility', as I have detailed, but it now seems unnecessary to continue with them throughout this dissertation.⁶

⁶ Bracketing tends to get tiresome, and as someone perceptively commented, 'the trouble with bracketing is that once you start an awful lot of the 'real-world' ends up in brackets'. I cannot find the original statement, or its author, but the sentiment is worth repeating.

1.3 *"Social Organisation" Versus "Social Construction": A Clarification of the Ethnomethodological Position*

Just as the term 'social organisation' resonates with ethnomethodological meaning, more generally, 'social construction' resonates with sociological meaning. But, even though ethnomethodology is a form of sociology, the latter term is not often used within ethnomethodology. In this section the difference between saying a phenomenon is 'socially constructed' and saying it is 'socially organised' will be considered, hence clarifying ethnomethodological inquiry and its use in this study.

To recap, the term 'social organisation' has been commonly used by ethnomethodologists to focus inquiry on how social order is put together from within actual social scenes. A similarly brief outline of the term 'social construction' now needs to be provided. To begin, note that there are two elements to 'social construction': first, 'social constructionism' (or 'constructivism') is a commonly used label for a set of related approaches within the social sciences; and second, there is the possible use of the term 'social construction' by social scientists who do not identify with the broader label 'social constructionism', but nonetheless use the term (in something of a vernacular manner).

Despite the two elements there is a good case for fitting both within one general approach to social scientific inquiry. Following Woolgar (1983), this can be called the 'mediative position'. In a consideration

of the relationship between reality and accounts,⁷ Woolgar sets up the mediative position as follows:

The position holds that there is nothing inherent in the character of real-world objects which uniquely determine accounts of those objects. ... Hence, the accounts can be thought of as products of the social, cultural and historical circumstances which *intervene between* reality and the produced account. ... These accounts are to be understood as actively constructed accounts, rather than passively received reflections of an external world, and they are to be understood in terms of the social circumstances which shape their construction.
(1983: 244, original emphasis)

In short, the mediative position still holds to the existence of an independent reality, but emphasises that the way we know reality - accounts - is mediated by social circumstances. Clearly, social constructionism fits within the rubric of the mediative position.

As an example of a social constructionist approach, consider the following statement about infertility.

Thus, the diagnosis of infertility, measured and studied by medical and social scientists, represents an accounting of the "failure" to conceive of those persons who are actively trying to conceive at the same time that they are socially designated as sanctioned to conceive. (Gerson, 1989: 49)

In this statement Gerson conceptualises infertility as a product of accounting work which is intensely socially mediated. The enclosure of "failure" in quotation marks, and the emphasis on the account being applied only to those "socially designated as sanctioned to conceive", being the key phrases. In short, infertility is presented as a 'social construct'.

⁷ Note that by account Woolgar refers to a broad range of entities: "words, utterances, conversational extracts, signs, gestures, objects, events and so on" (1983: 242).

Further, as a general approach, social constructionism is dominant within sociological studies of infertility.⁸ Some researchers use social constructionist arguments in an implicit way, while others are much more explicit, as the titles of the following research papers indicate:

- Infertility: The construction of desperation. (Gerson, 1989)
- The social construction of infertility: From private matter to social concern. (Scratchfield, 1989a)
- The infertility enterprise: IVF and the technological construction of reproductive impairments. (Scratchfield, 1989b)
- Deconstructing "desperateness": The social construction of infertility in popular representations of new reproductive technologies. (Franklin, 1990)
- Eager for medicalisation: The social production of infertility as a disease. (Becker & Nachtigall, 1992)

Whether the term is 'construction' or 'production' the emphasis is the same: infertility is mediated by social circumstances.

⁸ This dominance is not at all surprising, for at least two points. First, contemporary sociology has now firmly rejected 'objectivism' (eg. see Giddens, 1984), and constructionism fits neatly with this rejection. Second, social constructionism is currently very popular as an approach to the study of 'social problems' (eg. see Best, 1989; Spector & Kitsuse, 1977; Schneider, 1985), and this is where many researchers place the study of infertility (see chapter five).

This brief outline of the term 'social construction' will suffice, and we can now get to the crux of the matter - what is the difference between saying male infertility is 'socially constructed' and saying it is 'socially organised'? The difference is subtle, difficult to explain, and often misunderstood. Following from Woolgar's description of the mediative position, we can begin by considering his description of the "constitutive position".⁹ Here is one example of how he sets it out, again framed within a concern for the relationship between accounts and reality:

Both reflective and mediative positions contrast with a third position, that accounts are *constitutive* of reality. In this view there is no a priori distinction between accounts and reality; it is not that accounts reflect reality [the 'reflective position'], nor that they are mediated products of actors' attempts to characterize an actual reality while under the influence (so to speak) of their social milieu. Instead, the accounts *are* the reality; there is no reality beyond the constructs we imply when we talk of 'reality' (Garfinkel, 1952: 351). The important difference of this third position is that there is no commitment, implicit or otherwise, to the independent existence of any reality. (Woolgar, 1983: 245-245, original emphasis)

Thus, in contrast to the mediative position, which still holds to the existence of an independent reality, the constitutive position holds that "accounts are the reality".

At first glance, the implication appears to be that if accounts are the reality, then there is little point in talking of reality being socially constructed by situated actors, for, if all we have are accounts then surely everything is socially constructed. One could say that, but

⁹ Note that Woolgar is not an ethnomethodologist. His main area of interest is the social study of science, but in several places he has used ethnomethodology as a base for formulating a program of sociological study in this area (1981, 1983, 1988). More generally he uses ethnomethodology (mainly Garfinkel) to describe the "constitutive position".

ethnomethodologists do not. To clarify this point it is essential to point out that ethnomethodology does not espouse a naive idealist view that 'objects do not exist'. Rather, ethnomethodology draws upon a phenomenological position, which

... does not seek to *reduce* objects to appearances ... because it does not respect the contrast of 'object' and 'appearance' that was previously in place. The slogan ['there is nothing behind the appearances'] can now be seen to mean something quite different than that objects do not exist, that only appearances do. Rather than putting appearances where objects used to be, one may be seen to be drawing attention to the way in which (so to speak) *objects are found in their appearance*. The 'objects' have been 'relocated' and are to be found from amongst the appearances. (Sharrock & Anderson, 1991: 71, original emphasis)

Or, in simpler terms, "... social facts are neither 'in the head' or 'out in the world' but entirely 'within the (socially organised) practices by which they are assembled' (Sharrock, 1989: 673).¹⁰

Thus, there is no denial of reality, nor even a consideration of how reality is 'socially constructed'. Instead, ethnomethodology suggests a transferral of focus to the question of just how, in any social scene, the objectivity - the readily explicable nature - of that scene is available to members. We do proceed through our everyday life for

¹⁰ Additionally, the above elaboration shows why Woolgar's use of ethnomethodology has drawn some critical comment from ethnomethodologists. Button and Sharrock (1993) comment that Woolgar's representation of the constitutive position as

scientific knowledge -----> the natural world

merely reverses the relationship between language (a large part of accounts) and reality, and this is no longer a useful way to frame inquiry. They invoke Wittgenstein's argument that there is no determinate connection between language and reality, moreover, such questioning is itself an expression of confusion and is meaningless as "It is *intelligibility, not reality, that is relative to language*" (Button & Sharrock, 1993: 13, original emphasis).

the most part in the "natural stance", that is, assuming that as we see things so they are, but the whole import of ethnomethodology is that the naturalness of phenomena is accomplished with and within socially organised procedures. The suggestion is that it is 'social organisation' and not 'social construction' that is inescapable: "... every feature of sense, of fact, of method, for every particular case of inquiry without exception, is the managed accomplishment of organized settings of practical actions ..." (Garfinkel, 1967: 32, emphasis added).

A passage from Button and Sharrock is useful to give a concreteness to the above argument:

That, for instance, we have written this from left to right and that you will read it from left to right; that we follow the footpath sign by taking the direction marked by the pointed end; that when a friend tells us a joke we laugh, and do not fall on the floor ranting and raving; that we speak one at a time and not at the same time; that we greet people when greeted, etc. These are all examples of what Wittgenstein calls "natural reactions". We do not "negotiate" these matters, we do not, for example, speak one at a time because we have just agreed to. Neither do we greet you because we have "interpreted" your utterance as a greeting. This agreement, this consensus is embedded in our actions, there is a consensus in our actions, they are "natural reactions" by virtue of our participation in a language game. (1993: 15)

Ethnomethodology focuses on the orderliness of social life but does not use the possibility of "negotiation" or "interpretation" (or anything else) to claim that order is 'socially constructed'; rather the argument is that order is recognised and displayed, naturally, from within social scenes. Social activities are self-organising and naturally ordered and this is obvious to members by virtue of their membership of a culture.

This argument is exactly what Garfinkel established so well in *Studies in Ethnomethodology*, and it also explains his frequent talk of "naturally occurring organizations of ordinary activity" and the "essentially uninteresting reflexivity of accounts". Social order is put together upon a whole raft of taken-for-granted procedures, and the aim of ethnomethodology is to study the social organisation of these procedures in fine detail. Hence, the reason why this study is about the 'social organisation' and not the 'social construction' of male infertility.

1.4 *The Missing Literature Review*

So far, sporadic references have been made to infertility research but there has been no review of the social science literature on infertility. In fact, this study does not offer such a review. Given the accountable nature of such an omission, a justification will be provided below.

Within the infertility literature there are very few studies that focus specifically on male infertility (see Bents, 1985; Brander, 1992; Greil, 1991; Matthews & Matthews, 1986b; also see chapter five). Therefore, it would appear to be a simple task to review the few studies that do exist. However, the trouble is that there is a much larger literature on infertility in general, and much of this contains comment on matters related to male infertility. The broader literature on the general topic of infertility is derived from diverse disciplinary backgrounds including studies of a medical, historical,

anthropological, ethical, psychological, and sociological, nature. There are also studies with a social work and counselling focus, a nursing focus, and an extremely large body of feminist literature critical of reproductive technologies. If one was committed to reviewing the literature, it would seem necessary to cover both areas. But, while one could attempt to describe this diverse range of studies, literature reviews generally involve at least comparison and critique, hence, a review of the infertility literature would face the problem of reconciling the different bases of existing research. Without detailing these bases, it should be obvious that a serious review of this literature is a major undertaking.

The principle justification for not offering such a review is that given the framework of this study it is mostly irrelevant, or, of only marginal utility to the study. There are two points that elaborate this claim. First, at risk of over-generalisation, my feeling is that literature reviews reproduce a 'capture' model of inquiry. That is, a researcher diligently collects, reads, and critiques as much of the relevant literature as possible, with the upshot that lacunae and problematic areas are identified, which are then available as a focus in the next wave of research. The model is one of cumulative capture of phenomena. Obviously, research can indeed build up cumulatively - the body of work within conversation analysis being a good example - but what I want to emphasise is that it seems logical to conduct such literature reviewing only if the imminent study is viewed as fitting neatly within that body of literature. Surely, the flip side of showing familiarity with a literature is to stake a claim within that research area? If this view is accepted, then the problem for this study should

be clear: as an ethnomethodologically based sociological study of the social organisation of male infertility, it does not fit neatly into the existing body of research on infertility.

Certainly, the empirical chapters will include some discussion of existing research on infertility. But, and this is the second point of justification, what I wish to avoid in discussing such research is what Sharrock (1989) has aptly termed "ritual affirmations of difference". In discussing the 'ethnomethodological critique' of other forms of sociology, and the reaction of many sociologists to ethnomethodology, Sharrock uses the phrase as follows:

A critique which truly deserved that name would be one which raised its objections from a distinctly ethnomethodological point of view, but such a critique would, in my opinion, merely resemble the kinds of denunciation of ethnomethodology itself which I have earlier described as 'ritual affirmations of difference'. It would effectively decry functionalism (or whatever) for not being ethnomethodology. ... The most effective criticism is that which queries the viability of projects in their own terms, and the criticisms which can be most tellingly made by ethnomethodologists are not those which presuppose their own standpoint but those which take other sociological projects initially at face value and then look at problems in their principled implementation. (1989: 665-667)

Sharrock's argument usefully highlights the difficulty I have with a full-fledged literature review. Given that the infertility literature has not been touched by ethnomethodological insights, but this is what is utilised in this study, a review of the infertility literature would most likely end up as a "ritual affirmation of difference". The infertility literature would be decried for not being based on ethnomethodology - a pointless and negative option.

A more positive alternative is to make a literature review with a view to reconceptualising approaches to infertility. This could involve discussion of methodology and/or theoretical presuppositions.¹¹ While this option is more attractive than the former, it is not pursued here for the point that I noted in the introduction. That is, the emphasis of this study is on producing original empirical analyses of male infertility. The second more positive form of literature review would mainly involve non-empirical theoretical discussion - this simply is not the aim of this study.

In my view, these reasons constitute sufficient justification for not offering a review of the infertility literature. Additionally, it can be stressed that this is not a type of 'straw man' argument designed to remove the hard task of literature reviewing. I have read a great deal of the infertility literature, and am certainly familiar with its range and the major work within it,¹² and this familiarity will be displayed in the empirical chapters as research from the infertility literature is discussed and critiqued. However, this is not a form of literature reviewing, rather, it is very much a selective utilisation of the infertility literature where my aim is to engage with existing empirical research on infertility.

¹¹ For a good example of this kind of approach, see Gubrium and Holstein's (1987) evaluation and reconceptualisation of "family studies".

¹² Some of the more important research has come from those who have developed ongoing programs of research on infertility. The key researchers here, and examples of their work, are: Arthur Greil (Greil, 1991; Greil, Leitko, & Porter, 1988; Greil, Porter, Leitko, & Riscilli, 1989;); Ralph Matthews and Anne Matthews (1986a, 1986b); Charlene Miall (1985, 1986, 1989); David Owens (Owens, 1982, 1986; Owens, Edelmann & Humphrey, forthcoming); Naomi Pfeffer (Pfeffer, 1985, 1987; Pfeffer & Woollett, 1983); and Margerete Sandelowski (Sandelowski, 1986, 1987, 1990a; Sandelowski, Holditch-Davis, & Harris, 1990). There are other researchers engaging in ongoing research on infertility, particularly from psychological perspectives, but the above are the most sociological.

1.4 *Data Description*

Keeping with the theme of inclusion and exclusion, this section will provide details of the overall data collection process, and the selective use of this data. This section could have been placed in an appendix, but it is useful here because it provides a good indication of the nature of this study - a point to be developed in the final section of this chapter.

Probably not unlike most sociological studies, the data actually used in the empirical chapters represent a mere fraction of the total available. Briefly, the empirical chapters utilise the following materials:

- Ch. Two: extracts from interviews with infertile men;
 ethnographic details from an infertility support
 group meeting;
- Ch. Three: medical journal editorial;
- Ch. Four: ethnographic details from an annual meeting and
 conference of the New Zealand Infertility Society; a
 press clipping; extract from a novel; extract of
 dialogue from a film; extract from an interview with
 a donor insemination technician;
- Ch. Five: social science accounts of non-response in studies of
 infertility; this study's response/non-response rate
 for the interviews with infertile men.

Apart from the use of social science accounts as data, the materials used are derived from three sources: a collection of non-academic

textual material; ethnographic material; and interview material. More details are provided on each of these below.

Collection of Textual Material

Throughout the time spent on this research, written material on male infertility or infertility in general, was collected and a substantial amount accumulated. Sources include: local newspapers, popular magazines, novels, infertility society newsletters, and self-help books for the infertile. There was no formulated plan to use this in any specific way, but clearly some of this material has been used. Selection of texts to be presented and analysed occurred in an ad hoc way: a general topic was firmed up for discussion, often by using existing research as a starting point and/or from interesting observations from my own data, and somewhere in this process I would begin putting together material potentially useful for an analysis relevant to the topic of discussion. In a roughly consistent manner I tried to follow the ethnomethodological principle of approaching real-world materials in an unmotivated manner (any programmatic outline of ethnomethodology will include this as a principle). Obviously, this broaches the large topic of objectivity, indifference and so on (see chapter 6), but while I agree that "unmotivated analysis" is a problematic concept, the term is a useful gloss for the general principle followed. That is, I would try to avoid selecting materials because they 'worked' in the argument I wished to make; instead a quick decision to use the material was made and then analysis proceeded. Obviously, the material that has been used from this collection suits the specific arguments in each empirical chapter, but the point is there is no overarching principle of

inclusion/exclusion operating - other materials would have done equally as well. Also, this indicates that there is no sense in which analyses of textual material are meant to 'add up' to a theoretical statement on the representation of infertility in texts.

Ethnographic Material

This material is the least used of the three, but again constitutes a large amount of material. It was actually collected over a period of two years and involved many hours of participant observation and ethnographic notekeeping. But because I use this material only twice in the study, and then only briefly, there is no need to go into too much detail here. Basically, I attended meetings of three groups: a local infertility society (seven meetings); the annual meeting and conference of the New Zealand Infertility Society (two meetings); and a smaller support group with a primary focus on counselling (four meetings). I made it clear to the members of these groups that I was conducting research on infertility, and there was overwhelming support for this research to be done. I did not extend this support by broaching the possibility of audio-taping any of these meetings; instead, I relied on traditional pen-and-paper methods. While audiotapes of these meetings could have provided valuable data, I do not see this missed opportunity as a serious problem. This is because, firstly, such data would beg to be analysed with the social organisation of conference talk, support group talk, and self-help society talk in mind, hence this would extend the already conceptual sense in which the study is about male infertility. Secondly, this extra empirical material is not needed - there is no problem of a lack of data for the study as the small fragments that are used are more than

enough for my purposes. It is perhaps somewhat of a pity that more of the ethnographic material is not used, but my preference is to deal with the material that is presented, in as much detail as possible, and avoid the temptation to suffuse the study with references to other empirical material -a temptation which might give the study an anecdotal rather than analytical tenor.

Interview Material

Two sets of interviews were conducted as part of the study. I was the sole interviewer in all cases, with all interviews audio-taped with the permission of the interviewees, and all transcribed in full (by me). The smaller set (N=13) consists of interviews with people whom I grouped under the label 'health professionals'. This term is used approximately because not all were working within medical realms, but certainly all had in common a major involvement with infertility. They included: social workers who counselled infertile people; a urologist; gynaecologists, scientists, nurses, and technicians working in donor insemination units; and an ivf technician. Further information on these interviews is not needed as only one extract from this set is used in the empirical chapters (see chapter four). The second set (N=19) consists of interviews with infertile men, where this is taken to mean that a couple's infertility had been partly or solely attributed to "male factors" (to use a medical term). Several of the men actually had children, or were expecting children, but the main inclusion criterion was that they had, at some stage, been considered infertile. Again, because these interviews were in the end not used to a great extent - chapter two and chapter five use or refer to interviews with infertile men - there is no need to provide sociological

details (socioeconomic status, age, education level, etc.) on this group of men.

These three materials constitute the data corpus. In each empirical chapter a strong attempt is made to display the data that are to be analysed. This is because inquiry into the incarnate social organisation of phenomena calls for close attention to real-world practical activities. Of course, data are always only a trace of real-world activities, and the endorsement of fine-grained analysis is not some form of naive empiricism; it merely reflects the fact that some data are more useful than others for ethnomethodological inquiry. This brief description of the data will suffice, for the empirical chapters make clear the basis on which analysis proceeds.

1.6 *Final Introductory Points Developed From an Observation About the Data*

While the above section has provided a simple description of the data, it also indicates a more general feature of this study. In short, it shows that this study is something of a hybrid. This should be very clear from the details provided, specifically the fact that such a large amount of what could be called 'traditional' sociological data - interviews and ethnographic fieldwork - have been gathered, but so little use has been made of them. The natural question is, why go to so much trouble only to use the data in such a minimal way? The truthful answer is that this study did not begin with an ethnomethodological framework, rather the utility of such an

approach was realised in the process of doing it - the adoption of ethnomethodology occurred during the collection of interview and ethnographic material, and in considering how to analyse such material.

Obviously, if I considered this a serious problem the dissertation would not appear in the form it does. As it is, there are ample data to support the form of analysis offered and, perhaps more importantly, I do not see any problem in grafting an ethnomethodological frame of inquiry onto an initial substantive topic (ie. male infertility). I will detail why shortly, but firstly, by way of illustrating the point that some ethnomethodologists do consider hybrid approaches problematic, consider this conclusion from Maynard's appraisal of the relationship between language, interaction and the study of social problems:

This paper, however, is not an appeal for a theory of social problems that *incorporates* sociolinguistics, conversation analysis, cognitive sociology, ethnomethodology, or any other interactional inquiry, where one starts with a concern for an official social problem and looks for its interactional manifestations using one of these as a method for inquiry. It advocates further studies of language and interaction, for these are the stuff and substance of social life, and their investigation leads to a theory of the interaction order as an organized domain in its own right. ... Thus, the general answer to why language and interaction matter to the sociology of social problems is that their study permits the development of propositions about social organization. Although not about social problems traditionally conceived, those propositions have implications for the sociology of social problems, as they do for other substantive areas in sociology. (1988: 326, original emphasis)

Maynard seems to be offering two choices.¹³ Either, ethnomethodology remains a relatively pure domain of inquiry offering "propositions about social organisation" (or 'ethnomethods'), with the possibility of implications for other forms of sociology as a bonus. Or, ethnomethodology is incorporated in sociological inquiries which start with topics other than social organisation itself (eg. 'social problems'). For the sake of brevity, call the first the 'implications option' and the second the 'incorporation option'. Some very useful points can be made in reflection on these options to round off this introductory chapter.¹⁴

First, if we attempt to categorise what I have so far outlined of this study, it is not at all clear which option fits best. On one hand, the explication of the title shows that the study is more about social organisation than it is about male infertility as a 'social problem'. But, on the other hand, I see no need to totally abandon an interest in the phenomenon of male infertility (more on this below). Instead, this study aims to find a balance between the two, and this difficult task constitutes a tension which runs throughout this study.

But there is something more general to note about the two options: the very posing of such a distinction has a history. It is this history

¹³ Maynard's argument is chosen not because of any particular feature of Maynard's ethnomethodological work, but simply because this passage encapsulates one way of framing the relationship between ethnomethodology and other forms of sociology.

¹⁴ The final chapter offers a much more detailed discussion of a debate within ethnomethodology about matters related to these options, and therefore the following points are brief.

that Alec McHoul seems to have in mind in the following pertinent comment:

Consider the phenomena of ethnomethodology: everyday accomplishment and scenic practice, rule, technique and systematics ... and so on ... I have suggested above that these be seen as anything but independent of ethnomethodological discursive production. The question is: to examine that production or to perform it? And what criteria might be hauled into the argument by which we could choose between these? **It appears that 'choice' is not what goes on here - only training.** (1982: 110, emphasis added)

The passage reflects a very important argument that McHoul has developed, but discussion of this will be left until the concluding chapter. In the meantime all I want to take from it is the argument that ethnomethodology, like the phenomena it studies, is an accomplishment, a crucial element of which is "training". My feeling is that Maynard's espousal of the 'implications option' is a strong reflection of training. Beginning at least with Garfinkel (1967), there is a strong tendency within ethnomethodology to consider the enterprise as incommensurable with, and alternate to, conventional sociology, and this position is still being strongly argued (for the best recent examples see, Bogen & Lynch, 1990; Garfinkel 1988; Garfinkel & Wieder, 1992; Sharrock & Anderson, 1987; Sharrock & Watson, 1988). In my view, Maynard's conclusion reiterates and reflects this part of ethnomethodology.

While solid arguments can indeed be made about ethnomethodology's distinctiveness within sociology, there is a more dubious side to the 'implications option'. It is this: in assuming that the ethnomethodological study of social organisation and

ethnomethods can have implications for other forms of sociology, there is little or no mention of the possibility that the relationship might be reversed. That is, that other forms of sociology might have implications for ethnomethodology. There are a growing number of social researchers aware of this potential two way relationship,¹⁵ nevertheless, there does seem to be a hard core purist element still strong in ethnomethodology.

This purist element certainly produces cogent empirical research, which is frequently referred to in the empirical chapters. However, there are some non-empirical troubles associated with this element, as some comments by Cooper (1992) suggest. In his review of an important position statement for ethnomethodology - *Ethnomethodology and the Human Sciences* (Button, 1991) - Cooper noted the tendency of some ethnomethodologists to speak with "an almost embattled quality": they wish to proclaim the relevance of ethnomethodology to other forms of sociology, but at the same time they want to clearly define the boundaries so that their version of the enterprise can be "safeguarded from redescription or, worse, incorporation" (1992: 135). Cooper's point is that the offer of relevance to other forms of sociology has not been reciprocated -

¹⁵ In this study I rely most heavily on the work of Alec McHoul, but some of the more well known social researchers that could be included in this group are: Melvin Pollner, David Silverman, and Dorothy Smith. Also of interest is Drew and Heritage's recently published volume of conversation analytic works. Specifically, an introductory comment by Drew and Heritage emphasises the desirability of rapprochement:

Although the *methods* employed in the present studies are not always readily compatible with those of ethnography or survey research, the contributions to this volume sketch the kinds of possibilities that can emerge when CA techniques of analysis are applied to institutional interaction. It is in a spirit of openness to these future possibilities that the present volume is undertaken. (1992: 59, original emphasis)

talking and listening across the boundaries has mostly been one way.¹⁶

While I have certainly adopted ethnomethodology, and do find it an insightful sociological perspective, I agree with Cooper that there is no need for ethnomethodology to isolate itself from the rest of sociology. The beauty of my geographic isolation from a 'training group' of ethnomethodologists is that this study has had the potential to grow both in an ad hoc manner and relatively free (I hope) from a dogmatic style of ethnomethodology. To reiterate, the aim is to engage with existing sociological research on infertility and not to state a "ritual affirmation of difference". Starting from a hybrid position does enable this to a large degree.

Moreover, it is worth noting that there are an increasing number of sociologists engaging ethnomethodology with conventional sociology, and doing so in a way which avoids "ritual affirmations of difference". There is no need to discuss such work in great depth, but it is worth considering a list of such work in order to see the type of work being done. Here is a brief list, compiled in alphabetical order - the studies listed have:

- reviewed approaches to "television and audience activity", showing that television viewing needs to be approached as a practical activity (Crook, 1989);
- reconceptualised 'family', showing that it is not found in any one privileged place but can be studied as a feature of

¹⁶ See Frank (1985) for a more developed and critical discussion of points similar to those Cooper raises.

language-in-use which accomplishes varied projects (Gubrium & Holstein, 1987);

- studied "gender effects on involuntary mental hospitalization", describing these effects as situated accomplishments of commitment proceedings dynamically linked to the social organisation of such action (Holstein, 1987);
- studied the structure of interview talk in "careers counselling for the mildly mentally handicapped", showing that interviews are a problematic basis for placement, and suggesting possible directions for change (Hughes & May, 1985);
- studied "the delivery and reception of advice in interactions between health visitors and first-time mothers", emphasising that this activity is carried out through talk, and hence is subject to the constraining and enabling aspects of talk as a social organisation (Heritage & Sufi, 1992);
- studied "negotiating child sexual abuse", questioning the claim that investigative interviews are coercive, showing instead, through close examination of such talk, that children can be competent users of socially available negotiation mechanisms (Lloyd, 1992);
- suggested that "the getting of sexuality" is a problem within cultures which link adult status with sexual competency but fail to provide formal teaching mechanisms, noting, however, that there are many secondary systems of elaboration (McHoul, 1986);

- studied "the school's work of sorting students", showing that such work is accomplished through the local application of context independent mechanisms (Mehan, 1991);
- studied the organisation of counselling talk for AIDS patients, showing how interactants worked with and within communication formats, and suggested implications for both sociologists and practitioners (Perakyla & Silverman, 1991b);
- studied consultations at out-patient clinics for adolescent diabetics, showing that medical interaction is thoroughly imbued with moral concerns, but not of a fixed nature: participants to clinic consultations use shared resources to both construct and rebut moral implications of categorisation work (Silverman, 1987a);
- reviewed the "problem of human machine communication", showing that the design of interactive programs cannot be modelled without considering the situated interpretative practices employed by speakers, together, in natural conversation (Suchman, 1987);
- reconceptualised gender as a routine accomplishment embedded in everyday interaction (West & Zimmerman, 1987);
- studied phone calls to emergency service agencies, focusing on a single conversation where "words failed" with dire consequences, thus showing that calls for help go 'right' and 'wrong' in systematic, orderly ways (Whalen, Zimmerman, & Whalen, 1988).

This is not a comprehensive list, but short as it is, it shows a wide range of topics that are being addressed using insights from ethnomethodology. From, television viewing, family, health visiting, and mental illness, to, child sexual abuse interviews, stratifying students, sexuality, human-machine communication, and emergency phone calls. Also to be noted is that some of the authors of these studies would call themselves ethnomethodologists, while others would not. Irrespective of differences in self-labelling, what these studies exemplify is the growing use of ethnomethodological insights in a manner whereby links can be forged with conventional sociological study of substantive topics. This can take the form of reconceptualisation of study areas (eg. Crook; Gubrium & Holstein; Mehan; West & Zimmerman), or actual empirical study which has clear implications for the practical activities studied (eg. Peraklyia & Silverman; Heritage & Sufi; Lloyd).

There is also a more general point to be taken from this list. It is this: an incorporation or implications binary model of the relationship between ethnomethodology and sociology is outdated and unhelpful. Those trained in the view that ethnomethodology could proceed solely by making "propositions about social organisation" and being indifferent to substantive topics, have been overtaken by events - researchers are proceeding regardless of constraints suggested by 'training'. Moreover, it is not at all clear whether the studies listed above have started with 'social problems' and added knowledge about social organisation, or vice versa. But, surely, given ethnomethodological knowledge of reflexivity, it is not at all helpful

to conceptualise research in such a linear, additive manner. What is clear, is that the above authors have retained the ethnomethodological emphasis on fine-grained study, and have taken various 'theoretical' precepts from ethnomethodology, but have no longer remained 'indifferent' to their topic of study. Where there is a practical implication or a point of social critique that can be made, it is made.

This rejection of 'indifference' may have a great deal to do with the "recognition, acceptance, and even welcome from the neighbors" (Pollner, 1991: 370) that ethnomethodology is now receiving. Additionally, the acceptance and recognition that Pollner rightly identifies, has meant the inevitable "incorporation" that Maynard and others disfavoured. The upshot is that attempts to maintain a 'pure' strain of ethnomethodology are doomed, and it now seems pointless to claim that ethnomethodology is an "alternate" sociology. Certainly it is different, but not so different as to inhibit insightful and productive research from hybrid positions, as the above studies show.

There is one final point that can be developed from the above list. In general, these studies illustrate the argument that "there is no necessary contradiction in seeking to study *both* particulars *and* practices. ... [F]or analytic purposes and in real life, form and content depend upon each other" (Silverman, 1985: 172, original emphasis). That is, apparently 'abstract' formal knowledge about social organisation is centrally important for understanding substantive research topics. Moreover, such an argument has always been perfectly compatible with ethnomethodology. Consider this statement of the "symmetry proposal" by Heritage:

... CA [as a form of ethnomethodology] focuses on shared methods or procedures as the central resources through which actions are both produced and understood. The maintenance of these methods is a crucial feature of maintaining both the social organization of action and the social intelligibility of action. Both dimensions stand or fall together. (1988: 139)

Crucially, the social intelligibility of phenomena cannot be extricated from the methods or procedures which constitute actions - "form and content depend upon each other", "both dimensions stand or fall together".

It is this interdependence of form and content, particulars and practices, that makes this study of the social organisation of male infertility inherently sociological. And it is also why the study is able to maintain an interest in the substantive topic of male infertility, framed within knowledge about generic sense-assembly methods. The study of social organisation (form) does provide reciprocal information about the intelligibility (content) of male infertility. That, for example, when a stranger asks an infertile man whether he has any children, and he replies 'no, we can't afford it yet' (chapter two); that the fertility of semen is described by presence (percentage normally shaped, percentage motile) and not absence (percentage abnormally shaped, percentage immotile) (chapter three); that when we see a photograph of five men with musical instruments under the sign "Sperm Bank Five", we smile at the zaniness of the band's name (chapter four); that when an infertile male refuses to participate in a researcher's study of infertility, the researcher invokes the 'well known fact' that men find infertility a sensitive matter (chapter five), and so on; these "natural reactions" are substantive aspects of male

infertility embedded in social scenes as naturally organised practical activities. The intelligibility of male infertility and the ordered nature of the scenes within which it occurs "stand or fall together". In the following empirical chapters, attention is turned to explicating this relationship in more detail.

Chapter Two

'Do you have any children?' and the Disclosure of Infertility

2.1 *Introduction*

A major topic of discussion in the interviews with infertile men was how they told others of their infertility. Overall, it was very clear that 'infertility disclosures'¹ were highly variable events. They occurred between the men and their relatives, friends, and workmates, but also between the men and brief acquaintances or with people they had met for the first time. They occurred at parties, at work, at dinner, at family gatherings, and over the telephone. Special trips were made to make disclosures, but often they occurred relatively spontaneously, or were 'triggered off' by events like the adoption of a child or absence from work for a medical consultation or operation. Sometimes, they were made in anger at other people complaining about their children, but they were also made because of a professed desire to be open about infertility with other people. Infertility disclosures often produced an embarrassed silence or a disruption to the flow of conversation, but sometimes they resulted in a long and intimate conversation.

¹ This term will be more tightly specified below, for the meantime it refers to an infertile person's disclosure of their infertility to others. While this could occur through written discourse, here the focus is on disclosure in conversation.

In the major part of this chapter I will present some empirical material and use it to focus on a very small part of this variability of infertility disclosure. The focus is on the relatively common situation where unacquainted persons strike up a conversation, and as part of the exchange of questions and answers that routinely occur, the question 'do you have any children' is asked. Despite such a question presenting the possibility for an infertility disclosure and subsequent talk about infertility, my data suggest that this rarely happens. Using work from conversation analysis I pursue this issue in detail and by doing so provide an ethnomethodological slant on the disclosure of infertility. But, before this section a brief discussion is presented of two existing sociological models of infertility disclosure: a 'confession' model influenced by the work of Foucault; and a Goffman inspired dramaturgical model. These models attempt a much broader explanation of the variability of infertility disclosure. In the concluding discussion some problems with these approaches are noted, but, as indicated in the introductory chapter, the aim is to engage with this prior research on infertility, and not to discount it out of hand.

2.2 *The Beginnings of a 'Confession' Model of Infertility Disclosure*

The infertility literature contains the beginnings of a 'confession' model of infertility disclosure based upon the work of Michel Foucault. The usefulness of Foucault's work is plain to see: amongst other things, he presents society as a system of surveillance, where a

key part of surveillance is the encouragement of subjects (individuals) to speak, to confess. He argues that confession has become pervasive in modern life:

... the confession became one of the West's most highly valued techniques for producing truth. We have since become a singularly confessing society. The confession has spread its effects far and wide. It plays a part in justice, medicine, education, family relationships, and love relations, **in the most ordinary affairs of everyday life**, and in the most solemn rites; one confesses one's crimes, one's sins, one's thoughts and desires, one's illnesses and troubles; one goes about telling, with the greatest precision, whatever is most difficult to tell. (1990: 59, emphasis added)

For Foucault, confession is more than just a ritual of speech: it unfolds within a power relationship where the one hearing the confession has required, prescribed and appreciated it, and also has the power to "judge, punish, forgive, console, and reconcile" (1990: 61-62). It is also a ritual which produces modifications in the person who confesses: exoneration, redemption, purification, liberation, and so on. Foucault suggests that the confession has become institutionalised in a whole series of relationships: children and parents; students and educators; patients and psychiatrists; delinquents and experts (63). Hence, the pervasiveness of confession in modern life.

But despite the usefulness of Foucault's work, there has not been, as far as I know, a systematic application of the concept of confession to infertility. However, there have been more broader applications of Foucault's arguments to infertility, sufficient to suggest that there are the beginnings of a 'confession' model of infertility disclosure. Below I will briefly discuss these beginnings.

Pfeffer (1988) has used Foucault's concept of "biopower" in a study of pronatalism and sterility in England. She argues that sterility emerged as a problem within a new political concern for the welfare of the population - the fertility rate came to signify the Empire's political, economic, biological and moral strength. Within this context techniques for surveying the fertility (and infertility) of the population developed and this served the interests of the science of demography, whose practitioners used a paucity of information on (in)fertility to establish professional status. Pfeffer's argument remains at a structural level, but it is easy to see how such an argument could be extended to include a notion of confession. For one, demographic surveys of the (in)fertility of a population can be conceptualised as forms of confession within the overarching concern for "biopower". Secondly, the connection between fertility and the moral strength of the Empire could be translated into everyday-level moral pressures to confess fertility troubles. This could lead to an interesting search for archival material which records infertility confessions from this period (1900-1950).

A more developed use of Foucauldian concepts is found in Gerson's exploratory paper on "Infertility and the construction of desperation" (1989). She utilises a social constructionist viewpoint in arguing that

The whole discourse of infertility, as well as its "diagnoses" and "treatments," is a particular historic creation, formed at the intersection of women's bodies, women's life trajectories and contemporary medical technology and service ... the experience

of infertility, occurring within individual bodies, is subject to the shaping of the "social body" and the "body politic." (1989: 47)

In common with Pfeffer, Gerson suggests that the development of specialist knowledge of infertility is part of the overall process of industrialisation of human reproduction, and part of the specific disciplinary control of the body. In this light she suggests that what infertility doctors offer their patients is not so much increased treatment but "increased surveillance" (1989: 54).

It is in Gerson's discussion of the involvement of psychologists and therapists in infertility that we see the greatest scope for development of a confession model of infertility disclosure. She notes that the existence of infertility that could not be explained by physiological factors provided a fertile space for psychologists and psychiatrists, who set about researching psychogenic causes of infertility. Gerson suggests that responses by medical doctors debunking this research was an attempt to preserve their disciplinary domain. In time, the psychologists and psychiatrists rejected the possibility of psychogenic causes of infertility, but they nonetheless moved into infertility territory:

By delineating a practice that does not presume to heal a bodily condition, mental health professionals can use the diagnosis of infertility to safely enlarge their scope of practice without stepping on any toes. As one scholar of infertility wrote: "It becomes the job of the mental health workers to intervene in the emotional aspects of infertility and alleviate the stresses aroused by the condition." If the multiplication of discourse -the surveillance of the mind - can no longer presume to heal the body, it will be enlisted once again to heal the soul. (Gerson, 1989: 56)

Clearly, Gerson is picking up Foucault's emphasis on the disciplinary and constitutive effects of bodies of knowledge. In doing so, she does not specifically mention confession, but it is reasonable to suggest that a key component of "surveillance" is "confession".

Pfeffers and Gerson's work represents the clearest beginning point for a confession model of infertility disclosure. As it is, the model is mostly implicit, but making it explicit would only require a tighter focus and a continuation of the existing use of Foucauldian concepts. The actual minimum details of the confession model could be as follows. Within the multiplication of discourse on infertility, produced by the disciplinary development of modern medicine - both biological and psychological - lie institutional sites which require confession. Medical professionals determine who is infertile, and in this determination require confessions (eg. they interrogate infertile people about their sexual practices and often require them to record the frequency of sexual intercourse). The doctor may then recommend that the infertile people seek counselling from mental health professionals or social workers - if they seek to use reproductive technologies they will certainly be required to pursue counselling of some kind. The counsellors both require further confessions in their private sessions and encourage confessions in the public realm by recommending that the infertile disclose their infertility to others. On a more 'archaeological' front, the confession model could pursue changes in written records of infertility. Past confessional accounts framed within the medical or religious theories of the day, could be

compared with modern day personal accounts of infertility contained in popular magazines, newspapers and so on.²

Thus, there is little doubt that there is both, a wealth of empirical material available for the development of a confession model, and, a context within sociology of favourable reception for such a Foucauldian model. Some critical comments on the confession model will be offered in the concluding discussion.

2.3 *A Dramaturgical Model of Infertility Disclosure*

Within the infertility literature, Miall's work (1985, 1986, 1989) is the most concerted application of Goffman's dramaturgical perspective to infertility. Two of Miall's papers deal most directly with disclosure: "The Stigma of Involuntary Childlessness" (1986), which considers the stigmatising potential of infertility and its supposed involvement in infertility disclosures; and "Authenticity and the Disclosure of the Information Preserve" (1989), which extends the earlier paper and focuses on disclosure by infertile adoptive mothers.³

² See Franklin (1990) and Stone (1991) for two recent examples of infertility research which utilise Foucault to analyse mass media texts on infertility. Both are similar to Pfeffer and Gerson, but again neither focus explicitly on 'confession'. Franklin presents an analysis of "the social construction of infertility in popular representations of new reproductive technologies"; Stone does essentially the same but chooses to call it "contextualising". Both use popular texts about infertility, including personal stories (confessions) from infertile people.

³ Note that Miall's research focuses on female infertility. Although she attempted to study both infertile men and women, she met great difficulty in obtaining a sample of men willing to talk to her (see chapter five). Regardless of the fact that she only spoke to infertile women, her model of infertility disclosure seems equally applicable to men. Also, I am just as interested here in the general principles of her model as in its specific details.

Miall's method of study was based upon a simple tactic: "In order to explore initial patterns of disclosure of the decision to adopt, respondents were asked to reconstruct their disclosures" (1989: 286). She also asked respondents to imagine a "typical meeting" with someone unaware of their adoptive status. She then probed these accounts with so called WH-questions (Schegloff, 1992: 1300), that is, why?, who?, what? and when? The reconstructions of the disclosures and the responses to the WH- questions constitute Miall's data. The analysis of this data is informed by Goffman's theoretical concepts, mainly from *Stigma* (1963) and *The Presentation of Self in Everyday Life* (1959), and the broader framework of interactionism.

Specifically, the dramaturgical model of disclosure is characterised by its emphasis upon selves managing information and appearance in interaction to project and preserve their 'face'. This is extended by the notions of 'discredited' and 'discreditable': discredited refers to persons openly available as stigmatised (eg. someone without a nose); while discreditable refers to attributes that may be stigmatisable if uncovered (eg. Agnes' penis (Garfinkel, 1967)) and hence results in "passing", or conscious management to produce good impressions (Goffman, 1963). Miall characterises infertility as a 'discreditable' state which involves both, self labelling - "respondents themselves categorized infertility as discreditable, as something negative, as representing some sort of failure" (1986: 279) - and, the reaction of other people - "Self labelling occurs when individuals recognize that other people may label their particular attributes as discrediting or deviant if they learn of them ..." (1986: 279). Miall glosses the deviant potential of infertility as stemming from a pronatalist

normative order. Hence, the essential ingredients of Miall's dramaturgical model of infertility disclosure are: individuals concerned with their face, aware of societal norms, who work with socially mediated self-labels to disclose or not disclose in particular emergent social interactions.

In her 1989 paper Miall extends this view to give more attention to societal pressure to disclose. She does this because she sees a paradox in her data: the majority of her respondents were aware of negative attitudes about adoption in society, but these same respondents readily told others of their adoptive parent status. Miall chooses to explain this by considering the nature of "authenticity". Following Goffman, she argues that professionals involved in managing adoption utilise "advocated codes of conduct", that is, formal guidelines as to how adoptive parents should disclose their status. These codes have some force as failure to adhere to them may result in less favourable treatment by adoption professionals. Also, support groups for adoptive parents tend to follow a code that revelation produces authenticity. Miall argues that it is the fear of being labelled unauthentic by the professionals that encourages adoptive parents to disclose their status, and this is regardless of whether adoptive parents themselves think revelation is related to authenticity.

Thus, Miall's analysis of disclosure includes a social-control-by-institutional-professionals explanation. In this sense her argument is similar to the confession model, the difference being that her dramaturgical frame suggests social control has a self-ish driving

force - "to gain public acknowledgement of authenticity" (1989: 298). It is 'face' and not institutionalised power/knowledge that is the focus of Miall's model. Again, some critical comments on this model will be offered in the concluding discussion.

2.4 *An Alternative View of Infertility Disclosure Based on the Findings of Conversation Analysis*

To recap, the beginnings of a confession model of infertility disclosure suggested that individuals confess their infertility to surveillants/confessors who mediate the power/knowledge of discourses (eg. biological and psychological medicine). In the dramaturgical model pronatalist societal norms and institutionally advocated codes of conduct are societal forces which encourage 'face'-concerned individuals to disclose their infertility to others. In traditional sociological terms the confession model emphasises structure while the dramaturgical model emphasises agency and interaction: the former studies discourses and the confessing effects they produce, while the latter studies individuals maintaining 'face' - a product of identity, self labelling and societal norms - in their interaction with others.

Despite obvious differences, the confession and dramaturgical model are similar in that both remain indifferent to the socially organised nature of infertility disclosures as an exchange of talk. Both focus on how infertility disclosures reflect more general social relationships (discourse effects, 'face' concerns). While it makes little

sense to focus on lack of attention to this factor as a sole point of critique, there are good reasons why attention to the endogenous organisation of infertility disclosures is important for any sociological model of infertility disclosure. Below, by utilising some conversation analytic work and some data on infertility disclosure, I explicate the importance of an ethnomethodological shift "... from considering how social relationships determine the course of talk to asking what social relationships consist in, considered as exchanges of talk" (Sharrock & Anderson, 1987: 318).

Shortly, some data extracts will be presented, but prior to this a point of definition and a proviso need to be made. First, the definition. In the following discussion I wish to distinguish between an 'infertility disclosure' and an 'infertility telling'. 'Infertility disclosure' will refer to a one turn disclosure of infertility, as when in reply to the question 'why don't you have any children?' someone replies 'I'm infertile'. An 'infertility telling' may develop from such a disclosure and refers to an extended stretch of talk, involving two or more participants, where infertility becomes the central topic of talk. In the following discussion I focus on these two speech exchanges where it is an infertile male⁴ telling some other(s) that he is infertile (or is part of an infertile couple), and that this is a first time occurrence within ordinary conversation.⁵

⁴ Although my focus is on infertile males' disclosure, this is purely an effect of my decision to focus overall on male infertility. I am not suggesting that there are gender differences in disclosing infertility - there may be - but in the absence of evidence on this matter I would suggest that this chapter be read as focusing on held-in-common conversational resources for doing disclosing work, irrespective of the gender involved.

⁵ Defining 'ordinary conversation' is a complex matter which I do not wish to go into. It is sufficient here if it is understood to refer to conversation other than that

Second, the proviso. I want to emphasise that because of data limitations the following analysis is rudimentary and exploratory. I do not have audiotapes of naturally occurring infertility disclosures/tellings. Rather, my data are secondary accounts of infertility disclosures/tellings, and in this respect they are virtually identical in form to Miall's data discussed above. However, I wish to analyse my data differently to Miall: the extracts recounting infertility disclosures will be treated as very rough approximations to the real talk that occurred. Conversation analytic work has shown that even the smallest particles in talk are essential components of the work accomplished in conversation (eg. see Heritage, 1984a on 'oh'), and my data certainly do not provide details of the real talk at this level. However, I believe that they do provide details of the overall structural features of infertility disclosures and this is sufficient for my purposes⁶. Certainly, the fine-grained research of conversation

occurring in institutional settings, eg. medical consultations, counselling sessions, requested tellings at infertility society meetings, and so on.

⁶ A very relevant example of what can be done with less than ideal data is provided by Rodriguez and Ryave (1992). They present a conversation analysis of "everyday secret telling interactions" where the data are not audio or video taped, but are based on written recollections of secret telling interactions that Rodriguez and Ryave's students were part of. While noting the limitations of their data, they argue that it does "display the existence of a general organizational pattern" (299). It is exactly this that I believe my data also display.

A lot has been made within ethnomethodology, particularly conversation analysis, about this issue of 'naturally occurring' versus 'contrived' data. Clearly, the almost sanctionable preference within ethnomethodology is for the use of 'naturally occurring' data, but, thankfully, there are enough ethnomethodologists about who now reject such a hard and fast distinction. This study contains no explication of this matter, I would merely note here that as Rodriguez and Ryave's work shows there are conversation analytic studies using 'contrived' data, and more generally, there are good theoretical reasons why this is possible. On the latter, Alec McHoul is insightful, and has certainly been an important part of my realisation that a distinction between 'naturally occurring' and 'contrived' data is misconceived and analytically limiting (in specific see McHoul, 1982, 1987a, 1988b)

analysis can be used to add confidence to the speculative nature of my enquiry.

Data Extracts

In the interviews with infertile men one of the commonest events recounted was being asked within ordinary conversation whether they had any children. I would suggest that this questioning takes two basic forms: firstly, if asked by a stranger (or not well-known person) the question is some variant of 'do you have any children'; and secondly if asked by someone already known it is more likely to be 'when are you' or 'are you going to have children'. Different ways of eliciting the information can occur and even in a first meeting the second question could follow a 'no' reply to the first, and of course without access to naturally occurring data there is no exact way of knowing about this variation. But by utilising research from conversation analysis we can reasonably confidently fill in the circumstances of at least the first form of inquiry, which is focused on below. For the sake of brevity, this is represented hereafter as *any children?*. Here are three recountings of *any children?*.⁷

(M1: Hans)

H: ... you might have say a social situation, it still occurs today, they say "oh, did you have any children". I say "no", and then it's almost an anticipation that needs a bit of explaining. One person's reaction, just the other day was, "don't you like children?". I was never given the

⁷ The extracts from interview transcripts are presented in standard orthography and not in the transcription style of conversation analysis, simply because the extracts themselves are not being analysed in a conversation analytic manner. All names are pseudonyms, except M which stands for Michael, the interviewer.

opportunity to explain and it straight away categorises me as being, ah, somewhat different than I really am, and so it's a problem, it's something that continually requires a bit of ah - I don't have a problem with that, I just tell them why, that's a fact.

M: What do you actually say.

H: I say I'm sterile, it's a pretty short cut way of stopping the conversation but ah ...

(M18: Rick)

R: ... often people will say to you "how many children, have you got any children?", and you say "no", and they might sort of make a bit of a comment without thinking about it, because I mean the first question is quite natural, but then sometimes they um, they might say something like "oh, when are you going to?", and you have to say "well, when we were", you know, you say something like "well if I knew how to do it, I would" sort of thing, and they suddenly freeze, you know, I think probably even when they ask the question they probably suddenly think 'hell' you know ...

(M19: Allan)

A: ... it would normally occur perhaps on meeting people for the first time, or early on, you know, if they've got children you ask how old they are or whatever and then they'll say "have you got any children yourself?" and yeh, that's it, that's the opener, you know.

M: Right, what happens after that.

A: Um, I say "well, no we haven't, because we can't", or whatever, and I sort of say um, "I've had a medical problem which has meant that I've had to have chemotherapy which has, it's meant that we can't have children now", and generally that's as far as it has to go, yeh.

M: What do they say then.

A: Um, it's hard for them, I think, then, huh huh huh, yeh, they don't know what to say ...

The extracts show three examples of infertile men being asked some variant of 'do you have any children?'. We can infer that these questions occur in a first-meeting situation, or at least in conversations between people not long acquainted. The following discussion will cover, firstly, the general structural features of the question, secondly, replies and next responses, and thirdly, prefacing work and curtailed infertility tellings.

General Structural Features of *Any Children?*

A first thing to note about *any children?* is that it is indeed a question. As McHoul (1987c) argues, a question should be conceptualised not as a disembodied word or sentence, but as something that does questioning *work*. What a question turns out to be hinges upon how members utilise commonsense knowledge in the occasioned setting where the questioning work is done. To illustrate this McHoul (1987c: 461) offers the apparently straightforward question,

Did you feed the cat this morning?

which might receive two types of hearing, as follows,

That's not my job, it's *yours*, you creep.

or,

Yeh - tuna fish.

The latter reply typifies how a question can be heard as a straightforward inquiry, while the former reply typifies how a question can be heard as doing some 'negative' work on the matter, or upon the person asking the question (McHoul calls these Q- versus N-hearings). This relatively simple point is a useful way to begin

explicating *any children?*, because what it points to is a realisation that questions are powerful social devices, and as such will be located in particular places within the overall organisation of talk.

Consider the possibility of the solitary utterance 'why?' being the reply to *any children?* Obviously, this indicates an N-hearing has been made, but it is perhaps just as obvious that we would not expect to hear any of the infertile men uttering this response in the contexts recounted above. This is because the question occurs between non-familiars who, as such, face the pressing practical problem of coproducing topical talk, and within this context, we would not expect a 'why?' reply because *any children?* is not accountable in that way. A brief outline of conversation analytic research on topic production will make this point clearer.

Once greetings and introductions are exchanged, and perhaps after some initial 'make-talk' about the setting of the conversation or the weather, first-meeting conversationalists often move on to two types of question-answer pairs: "categorisation sequences" and "category-activity sequences" (Maynard & Zimmerman, 1984). Categorisation sequence refers to the everyday practice of asking questions about the characteristics of people, for example, where you live, how old you are, if you are married, and so on. The category-activity sequence is similar, but the questions here focus upon activities that people do and how these might categorise the people doing them - asking a person's occupation would be an extremely common example. There are various methodical ways in which these pre-topical sequences may or may not lead on to topical talk (see below) but, typically,

several questions from a categorisation or category activity sequence are offered before conversationalists take up extended topical talk.

Given the way in which questions can be dangerous because they may be heard in multiple ways, it might seem surprising that in conversation between non-familiars there is a wholesale exchange of questions (ie. pre-topical sequences). But, as Maynard and Zimmerman argue, the important point to note here is that the pre-topical sequence displays sensitivity to coparticipants. The questions are precisely **pre-topical**: conversationalists should orient to them as, if you like, means to getting to the 'real' topical talk and not the 'real' topical talk in itself. This is the sense in which *any children?* is (often) not accountable in the 'why?', or N-hearing way. It is understood by conversationalists that if they do not want to pursue a proffered topic then that can be signalled in various ways (eg. by minimal response), and other participants should be able to act upon those signals without difficulty. It is in this sense that Maynard and Zimmerman make the strong claim that the methodical ways in which topical talk evolves from pre-topical sequences "... are *required* forms for initiating autobiographical talk; they allow that talk to be produced *properly* and with *sensitivity* to co-participants ..." (309, original emphasis).

Clearly, *any children?* is a possible part of a pre-topical categorisation sequence - it tries to establish biographical information upon which to make topical talk. Within a pre-topical sequence the question is likely to be located after some prior questions: a questioner will have to ascertain that the recipient is a candidate for

parenthood before the question can be sensibly made. One can imagine previous talk establishing marital status (or partnered status), stage of life (eg. settled down with a home etc.), and attributions of age and sexual orientation. In this sense the *any children?* question is "recipient designed" (Sacks, Schegloff & Jefferson, 1974) using the knowledge built up in the conversation before topical talk occurs. It may also suggest that the question occurs at a point somewhat removed from the initial anonymity of the beginning of a conversation. Even the minimal exchange of information that is required to be able to make *any children?* moves the conversationalists away from absolute anonymity.

Replies and Next Responses

So far I have discussed some general features of *any children?* It may occur within pre-topical sequences in talk between non-familiars, where it most likely is positioned after initial questions which enable it to be made, and it partakes of the general sensitivity to other which pre-topical and topical packages enable. We can now consider possible replies and next responses.

One apparently simple way of replying is with a solitary 'no', and in extracts M1 and M18 there are indications that this occurs. If the men did indeed make this solitary utterance reply it would be very important for the shape of the subsequent interaction. It stands in direct contrast to a long form reply which is a clear indication of a "topic initial offer" (Maynard & Zimmerman, 1984). It is also different from a typical device for rejecting an invitation to talk on a

possibly sensitive topic, that is, a short-form reply plus a return question, as in the following example:

1. A: Are you a freshman here?
 2. B: Sophomore. Are you- what are you
 3. A: uh, I'm a freshman
- (Maynard & Zimmerman, 1984: 306)

Maynard and Zimmerman suggest that by offering a minimal reply and return question, recipient can clearly formulate a reply as only an "answer" and not an offer to do topical talk. In the above case, possible sensitivity (for either party) stemming from the different levels of the students is not taken up in topical talk. We might hypothesise then, that a reply such as 'No, what about you?' to *any children?* would, while relevancing an answer about the questioner's parental status, shut down the topic after the next response, or at least take topical talk away from the question of why the infertile male has no children.

However, by itself the 'no' reply does not seem to shut down the topic. Maynard and Zimmerman suggest that short form answers without return questions are equivocal as to whether they are a topic-initial offer. They are certainly not so clear an indication of taking up topic as a long form offer, but they may contain an implicit offer to take up the topic. Or alternatively, as Heritage (1988) shows, the 'no' reply may result in questioner *seeking* an account. He notes how when second speakers cannot accomplish a projected action they usually produce an explanation or account of why. When this is not forthcoming the first speaker will evidence a belief that one is due by overtly pursuing an account or explanation. Heritage gives the

example of two people talking, one announcing a failed exam to which the other asks:

G: So yih g'nna take it agai:n?=
 S: =nNo.

(O.5)

G: No:?

S: No.

(0.3)

G: Why no:t.=

S: = .t.hhhh I don't rilly wan'to.

(1988: 134, transcription simplified)

After the initial short form reply, there is a long delay while questioner waits for an explanation before prompting it with a return "No". The next response is no improvement, questioner waits again for an elaboration, none appears, and so finally questioner makes an overt request for an explanation.⁸ Overall, Heritage suggests that when questioners and recipients uncover an unexpected action (eg. failing an exam) account-giving is a normatively required feature of the subsequent talk.

It seems reasonable to include childlessness as an unexpected state for adults, and the fact that requests for accounts follow a 'no' reply to *any children?* suggests this. Thus, while mindful of data limitations, one common development following *any children?* can be represented as follows:

⁸ See Button and Casey (1985) for similar empirical examples. Sacks also made a similar point in his discussion of "diagnostic sequences": an important feature of the use of greeting substitutes ('how are you', 'y'alright') is that it is the recipient who initially directs the following interaction. If one replies 'fine' then the floor is handed back to the questioner, if one replies 'lousy' then the questioner is 'required' to pursue enquiries into 'how come' (Heath, 1981, summarising Sacks, 1975).

pre-topical inquiry	[Do you have any children?
minimal reply/equivocal topic initial offer	[No
wait for account	[... (probable pause)
request for account	[Don't you like children? When are you going to? etc.

Prefacing Work and Curtailed Infertility Tellings

The specific talk in the extracts above, and the talk in my interview data in general, suggest that when a disclosure of infertility is made in response to a request for an account, the conversation does not continue to an infertility telling. Regardless of whether the disclosure is relatively direct - "I say I'm sterile" (M1) - or euphemistic - "well if I knew how to do it, I would" (M18) - the result seems to be an embarrassed silence, stammering, 'ohs', or 'I don't know what to say' statements. In short, an infertility telling is curtailed.

This is an interesting outcome, particularly given the arguments of the confession and dramaturgical models. The general tenor of the

confession model is that people confess on an everyday basis, therefore, presumably they have little trouble in confessing. Why then, the curtailed infertility telling? In contrast, the dramaturgical model with its emphasis on 'face' and stigma clearly suggests that there are good reasons for disclosures being avoided. That is, infertile people may choose instead the activity of 'passing' -selective information management designed to avoid potential stigma. But, as noted, Miall's respondents disclosed their infertility or adoptive status quite freely, and Miall suggested this stemmed from a notion of 'authenticity' imposed by powerful institutional agents. However, in the above data extracts there is little evidence of passing⁹, and rather than explain the ease or difficulty of disclosing infertility by reference to outside social forces, as Miall has done, I wish to continue emphasising how talk as a structure-in-action constrains and enables infertility disclosures.

To precis the following argument, I believe that curtailed infertility tellings develop from the 'no' reply to *any children* because 'no' is insufficient to set up an infertility telling. To do an infertility telling one needs to forewarn questioner that talk is moving from business as usual, and the relative safeness and anonymity that permits, to business that may require an initial display of "troubles-receptiveness" (Jefferson, 1980) and further moves toward intimacy. Within the constraints of pre-topical talk the 'no' response cannot

⁹ There is some mention of 'passing' in the interview data. One man commented that when asked *any children?* he would reply with something like 'well, we're working on it'. I asked how long such a reply could be kept up and the obvious answer was 'not a lot longer'. Once a 'pass' is made to the effect that 'one is working on it' this provides the questioner with a future return to the topic, all of which multiplies the normative accountability of replies to *any children?*

accomplish this prefacing work and the talk moves on. Below I outline the basis of this argument, beginning with a brief outline of the development of conversation analytic work on prefacing.

In his examination of storytelling Sacks (1989) noted the importance of "story prefaces". As stories take more than one sentence to tell, a potential problem occurs at the end of the story's first sentence: this is a common place where another speaker may begin a turn at talk and if this occurs the story's telling may be preempted. One effective way to overcome this problem is the use of a preface. For example, in Sacks' data the prospective teller prefaced his story with, "You wanna hear muh-eh my sister told me a story last night" (1989: 338). The preface displays an intention to tell a story and also gives the potential hearers an indication of the source. As a device a preface maintains the ability of others to take a next turn, but it enhances the possibility of coparticipants reselecting the prospective teller to tell the story; and during the course of the story, coparticipants will not treat each possible turn completion as a place at which to take a next turn, but will offer indications that they are following the story.

Developing on the recognised importance of prefacing work, subsequent work in conversation analysis has examined, for example: how talk on topic is pre-sequenced (Button & Casey, 1985; Maynard & Zimmerman, 1984); how requests of action by others are prepared with statements such as 'can I ask you a question' (Schegloff, 1980); how "troubles-talk" displays an elaborate pattern of opening, talk on the trouble, and subsequent closing and movement to next topic

(Jefferson, 1988); how the giving of bad news can be so organised that the recipient of the news turns out to be the one who actually says it (Schegloff, 1988b). The general conclusion is that in any talk-in-interaction where participants will take an extended turn at talk the "preference" is for inclusion of prefacing work (on preference, see Bilmes, 1988).

A major function of this prefacing work is to maintain sensitivity to others and keep interaction at an affiliative as opposed to disaffiliative level (Jefferson, 1988; Schegloff, 1980; Heritage, 1984, 1988). A further important point to note about this line of research concerns intentions or motivations of the individuals party to the interaction. Conversation analysis is not concerned with the issue of whether conversationalists come to a interaction with an intention or motivation to tell a trouble, give bad news, tell a joke, disclose infertility, and so on (see Heritage, 1990/1991). The point is, regardless of any attribution of intention or motivation, individuals taking extended turns at talk in ordinary conversation will have to fit into the there-and-then constraints of talk. Moerman and Sacks have put this well:

... conversational sequencing is built in such a way as to require that participants must continually, there and then - without recourse to follow up tests, mutual examination of memoirs, surprise quizzes and other ways of checking on misunderstanding - demonstrate to one another that they understood or failed to understand the talk they are party to. (Moerman & Sacks, quoted in Drummond & Hopper, 1991: 305)

Members, by virtue of their membership, are expected to orient to the placement of their utterances within an overall stream of talk;

Moerman and Sacks' statement adds the important point that accountability for such orientation is instant, "there and then". Prefacing work is precisely a way of providing within talk an indication of what is coming up in the talk and how it should be understood on an ongoing basis.

The way in which a lack of prefacing work might be involved in curtailed infertility tellings can be seen by considering one example of an extended infertility telling. In response to an interview question about disclosing infertility to workmates, Peter recounted the following event.

(M2: Peter)

P: Well I was lucky, one day, this is where I say that it doesn't affect me, actually, vividly, I can remember one day I was at work, and it was a real bad time for both of us, I was at work, it was probably about ten o'clock in the morning and the manager was there, and he said to me "oh, what's the problem?". Because normally I just carry on, laugh and have a joke, but he knew there was something wrong, and we talked about it, and bang, I just burst out crying, told him the whole story, you know, he kind of locked the door, no-one to come in, and we talked about it for a good hour, and I think that's what, that was my release, you see that's why I say you close doors, you don't think of it until you need to start talking ...

In the interviews I did with infertile men it was very uncommon for men to report having long conversations with anyone except their partners about infertility, but here Peter recounts how an apparently simple question - "oh, what's the problem" - led to an extended infertility telling.

As a secondary account the data are obviously limited, however, by considering it in relation to conversation analysis research on "troubles-talk" (Jefferson, 1988) we can relatively confidently identify some features of a larger package for doing extended talk on troubles. Through a detailed study of ordinary conversations where people talked about their troubles Jefferson (see 1988 for details of her research programme) concluded that troubles-talk is a socially organised "package" with standard parts which occur in a standard order. She noted how "troubles-talk is so arranged that a coparticipant need not know about the presence of a trouble to effectively initiate talk about it" (1988: 421). For example, by using a "downgraded conventional response" to a greeting substitute a speaker can orient a coparticipant to a trouble, as follows:

A: hh How've you bee:n.

B: hh Oh:: surviving I guess, hhh!

(Jefferson, 1988: 422, transcription simplified)

This is one way in which questioner is alerted to the possibility of extended troubles-talk.¹⁰

Clearly, in Peter's account the manager is alerted to the possibility of trouble by Peter's non-routine behaviour: "... normally I just carry on, laugh and have a joke, but he knew there was something wrong ...". While behaviour in itself could lead to a "troubles inquiry" or an indication of "troubles receptiveness", it is quite likely that Peter also

¹⁰ There are a variety of next responses and connected ways that the talk may then develop, from relatively neutral responses like "continuers" ('mmm', 'yeh'), which may require further work by troubles teller to lead to troubles-talk, to responses that clearly indicate questioner's "troubles receptiveness" (eg. 'oh no', 'oh shit') and which greatly encourage subsequent troubles-talk (1988: 423-425).

displayed his troubles in the form of his talk, perhaps making a "downgraded conventional response" to a 'how are you' greeting substitute. Either way, as a signal that something is not right, both non-routine behaviour and non-routine forms of talk are efficient prefacing devices for a troubles telling.

It is interesting to note that Peter contextualises this extended infertility telling by stating it occurred at a "real bad time", a time where he had a "need to start talking". This provides a readily understandable motivation or intention for the infertility telling. There is no reason to question his account on this point, but we might add to it though, that even with a motivational 'need to start talking' if the right conversational devices are not used an infertility telling will not get done. The right things need to be said by questioner and recipient so that each can recognise that there is a trouble to be told. This is why prefacing is such a common device: it lets the recipient of an extended turn at talk know that a certain type of extended talk could be coming up,¹¹ with concomitant interactional requirements and obligations.

In contrast to Miall's dramaturgical model which emphasises the stigma that the discloser may face, the important point gained from considering conversation analytic work is that **both** questioner and recipient have selves they are concerned for. Both are at risk in talk, and not solely because any of the parties have stigmatisable information preserves, but also because of the expectation of

¹¹ This can work to the extent that the putative recipient of bad news etc. actually guesses what is to be told (see Schegloff, 1988).

competent use of conversational structures. This competence includes general knowledge of just where and when an activity like disclosing infertility can be accomplished. Clearly, infertility cannot be disclosed in just any conversation, let alone in pre-topical talk between non-familiars, because of the contingency that if bad or delicate news is volunteered then the recipient of the news may be unprepared to fulfil the interactional obligations of being a recipient to that news (Button & Casey, 1985).

Interestingly, on the other hand, my data suggest that by answering *any children?* with a minimal response recipients may find themselves at the receiving end of active pursuance of an account. That is, answers which are not infertility disclosures lead to account pursuances which may bring such a disclosure. I suspect that this situation may have a lot to do with why, after the second question which pursues an account and the response disclosing infertility, the conversation breaks and an infertility telling does not eventuate. The delay and embarrassing silence may be a time where the questioner reflects upon the very means whereby his or her account pursuing elicited a disclosure of infertility without any prefacing work by the recipient. The apparently simple utterance *any children?*, is retrospectively constituted as an insensitive one: when uttered it appeared as an information elicitor which could lead to topical talk, but three turns later its meaning is substantially altered and it has become a question which has set up troubles-talk without any prefacing work by the infertile male, hence, the questioner being 'lost for words'. As Heritage has noted, speakers are treated as having the capacity to produce or avoid producing the appropriate packages for

eliciting accounts, and "they may thus find themselves being accountable for and evaluated in terms of how they design and package their utterances" (1988: 138). This is precisely what seems to happen, and further, the participants to the conversations are aware of the accountable nature of their talk: Rick indicates that the questioners "suddenly freeze, you know, I think probably even when they ask the question they probably suddenly think 'hell', you know" (M18).

This is the risky nature of conversation, and particularly so for pre-topical talk amongst non-familiars. In the business of working up topical talk questions can be asked that inadvertently set up accounting procedures which may produce accounts that the mechanics of pre-topical talk are not well designed to cope with. As Jefferson's research shows, it requires a large package to cope with the large movement from business as usual to the reciprocal intimacy of troubles-talk. If even these large packages are prone to disruption by the constant pressure towards business as usual, how much more so for the pre-topical sequence which is precisely centred upon establishing what business as usual -coparticipants' autobiographicals - is to be in the talk that is getting done.

Thus, the apparently innocent question 'do you have any children?' can have major unintended consequences. The question seems to be aimed at initiating topical talk on children, but when in reply to an account pursuance, a 'truthful' disclosure of infertility is made, it seems to curtail the possibility of talk on that topic - in fact the possible topic has moved from children, to not having children.

Following Heritage, it can be argued that although members' knowledge of the proper use of conversational devices is usually taken-for-granted, when the devices are not used in their proper way, members are accountable for this improper use. In the case of the result of *any children?*, the questioner stammers and pauses - the apparently simple question they uttered turned out to do 'insensitive' questioning work.

This phenomenon is not likely to be isolated to infertility as the content of what is being disclosed. Extract M18 includes a comment from Rick on exactly this point:

it's one of those questions like "what do you do for a living", it used to be quite normal but now you have to think quite carefully about it because the person might have to say "well look, I used to work for IBM but I've been unemployed for two years and I'm forty five and I don't think I'll ever get another job".

In this case however, the long form reply could function as a topic initial offer and extended talk on the topic looks likely to occur. In the case of the hypothetical form of the curtailed infertility telling sequence, the movement from minimal reply, to account seeking, to infertility disclosure, happens too quickly - the pre-topical package does not seem capable of handling the correlative movement from relatively anonymous talk, to talk of a much more intimate nature.

Briefly, before moving to the concluding discussion, there is one other piece of empirical material worth considering. At the first infertility support group that I attended, the first part of the meeting was taken up with the participants introducing themselves to the

group (nine people). When the turn of one man came he gave his name and then immediately stated that he was infertile. The disclosure produced an almost visible shudder, a definite feeling of unease, in the group as a whole, and the man then continued without responding comment from anyone else. I believe this example illustrates how the preferred conversational devices for doing a personal disclosure are recognisable in their very absence. The man failed to allow a disclosure of infertility to come 'naturally', that is, with prefacing work, and the reaction of the others signalled that this was indeed missing. It is important to note that this occurred in a context within which personal disclosures might be an expected occurrence, but even within such an environment there are right and wrong ways to disclose personal troubles.¹²

2.5 *Concluding Discussion*

The above discussion has covered a wide range of divergent material, hence it might pay to reiterate the proviso noted in section 2.4. That is, my discussion must be considered speculative as it is dependent upon limited empirical material. The secondary recollections of real talk cannot reproduce the fine grained detail of the original talk, and therefore my analysis of the curtailed infertility tellings could be spurious - after *any children?* talk may have gone on to extended topical talk on infertility. However, the utilisation of

¹² There is a good chance that the 'right way' is the way it is done in ordinary conversation, that is with prefacing work, and within a more formal setting this device becomes the bedrock form, with departures from it being 'observable and reportable'.

existing conversation analytic studies, which are (usually) based on actual instances of talk, provides valuable insights into the overall organisation of talk within which *any children?* is located. Thus, although speculative, my discussion has a sound base. Using this base I explicated the general structural features of *any children?*, showing how such an apparently simple question is part of the complex machinery of talk. I emphasised that infertility disclosures were a conjoint accomplishment involving normative aspects in both, the pursuance of accounts, and in the very way members are accountable for their use of forms of talk. Additionally, I suggested that infertility tellings would require some prefacing work, and this was an important reason why when *any children?* is asked within pre-topical talk it does not lead to extended topical talk on infertility.

More generally, I think I have made a strong argument for approaching infertility disclosure by considering the endogenous organisation of talk. For, in disclosing infertility through talk members utilise a "... procedure through which participants constrain one another, and hold one another accountable to produce coherent and intelligible courses of action" (Goodwin & Heritage, 1990: 288). The specific intelligibility of male infertility known through infertility disclosures is imbricated within talk as a social organisation - it cannot be separated from an already social course of action.

Such an approach is certainly in contrast to Miall's dramaturgical model of infertility disclosure. She failed to consider the social organisation of disclosure talk itself, preferring to use accounts of disclosure as a resource for locating 'face-work', 'authenticity',

'information preserves' and so on. Clearly, a great deal of this approach is derived from Goffman, but it should be emphasised that Miall totally neglects the relevance of Goffman's work on 'forms of talk' (1981, 1983). As Maynard and Zimmerman's work shows, the general thrust of Goffman's work can be utilised in a different manner:

... rather than approaching relationships as a reality lying behind and influencing members' face-to-face behaviour¹³, we can investigate them for how, in the course of time, they are accomplished within everyday interaction by various speaking practices, including those involved in the production of topical talk. (1984: 305)

The contrast is also summarised nicely in a distinction that Watson (1992) notes: the dramaturgical model is concerned with 'self' whereas the ethnomethodological approach is concerned with 'member'. The former has a social psychological focus on individuals and impression management while the latter is concerned with the held in common resources that individuals use and not the individuals themselves.

This contrast probably has a lot to do with the celebrated divergence between Harvey Sacks and his initial teacher, Erving Goffman (see Schegloff, 1988a). Here an answer that Sacks gave to the question of why he sought the "apparatus rather than what people are trying to do in a conversation?" (Sacks, 1987: 67) is most germane. To

¹³ This is the essence of Miall's approach, and at times it leads to strongly essentialist statements. For example, at one point she comments that "...the apparent existence of a self-labelled, discreditable identity in the absence of visible deviant behaviour offers additional support for the concept of deviance as an inner essence which can exist independent of behaviour ..." (1986: 280). Miall fails to note that this supposed independence from behaviour is only known about through the process of interviewing respondents about infertility disclosures; in other words, the behaviour independent "inner essence" is known through the social behaviour of research interviewing.

paraphrase Sacks and apply his logic to infertility disclosure, you cannot find what people are trying to do by disclosing infertility until you find the kinds of things they work with, that is, social resources. If the system had a built in device for producing disclosures and you did not know about it, then you could be calling these devices 'face-work' or 'information preserve management' when they were perfectly well accounted for by the devices. What people want to do by disclosing infertility is another question, and it may be that detailed knowledge of conversational machinery will be sufficient as an account of what people are trying to do by disclosing infertility (see Sacks, 1987: 67). Thus, analysis begins with detailed consideration of the conversational machinery through which infertility disclosures are accomplished, and it is this line that I have followed, albeit in a speculative way.

Although my discussion of the confession model was brief given that it exists only in a seminal state, it should be clear that my approach is also in contrast to this model. It is now a well known point of critique that Foucault's analyses are heavily dependent upon textual analysis, that he tends to neglect the contingency of discourses, and presents, without sufficient analysis, discourses as having general social effects (eg. see Hepworth & Turner, 1982; Turner, 1984). The existing use of Foucauldian concepts in infertility research presents no exception to this tendency. While I do not deny the theoretical interest of a confession model of infertility disclosure, my analysis has some contrary implications for it.

Firstly, the findings of conversation analysis and my own speculative findings have a strong bearing on the claim that modern Western society is a confessing society. That is, a society where confession plays a part in the "most ordinary affairs of everyday life" (Foucault, 1990:59). If we assume that we can include ordinary conversation here, then my discussion suggests that there are some illnesses and troubles that, despite readily available questions leading to them, are not readily turned into extended telling of troubles (ie. 'confessions'), and this irrespective of the existence of a considerable machinery for prompting accounts. Hence, anyone serious about taking up Foucault's argument on confession needs to avoid making blanket statements calling any piece of discursive work a confession, or saying that they are a pervasive part of ordinary conversation. Doubts about a confession model were made very clear in the way in which the questioners who asked *any children?* were themselves made accountable for the way they had designed their utterances. A simplistic adoption of a confession model forgets that "interrogators have no guarantees" (McHoul, 1987c).

In addition, there are difficulties with applying the confession model to apparently well developed exchanges where people tell others of their illnesses and troubles. Research by Jefferson and Schegloff suggests that even extended confessions (of any subject matter) may be prone to collapse or divergence. A particular point that Jefferson stressed in her study of troubles-talk was that although the troubles-talk package is elegant it is also weak:

... it is constantly encroached upon, and recurrently breached, by the pressure towards business as usual, to which talk about a

trouble appears to be irrevocably vulnerable, and to the concerns of which a "trouble" appears to be irremediably subordinate and accountable. (1988: 440)

Because ordinary conversations are seldom a monologue and require display of attentiveness of all parties to what is being done in the talk, there are innumerable opportunities for the track of talk to move from a 'confession' to ordinary matters.¹⁴

Schegloff (1980) makes a similar point in his discussion of prefacing work. If a speaker sets up a coming extended turn at talk with a preliminary to a preliminary (can I ask you a question), and then a preliminary (details which fill in the context for the next action), before the action projection that it all leads to (can you do something for me), then there is a large amount of material which the recipient can take up as "something in its own right". Whatever this "something in its own right" is, may well be something to which a speaker did not attend, and thus the speaker may be puzzled about recipient's reaction to the preliminary work, with the upshot that elaborate repair work results (see Schegloff, 1992). There is good reason to believe that if 'confessions' do occur at the level of ordinary conversation, and Foucault suggests they do, then they will involve prefacing work, and in this work the problem of attending to the prefaces as "something in its own right" may occur. Again, confessions may therefore inevitably go off track and return to 'business as usual'. In simple terms, a 'confession' will not be established by the exercise of power relations which exist prior to the

¹⁴ And, of course, as ethnomethodologists have argued for a long time, these 'ordinary matters' are also an accomplishment, perhaps the most pervasive accomplishment of everyday life (eg. see Sacks, 1984; Pollner, 1987).

talk, but must be accomplished conjointly by speakers in conversation using the resources they have at hand.

However, the above points are consonant with one aspect of the confession model. If confessions are difficult to achieve in ordinary conversation, then this may provide an impetus for the proliferation of institutional sites for confession - counsellors, psychiatrists, and so on. This in turn could be linked to the ritualistic aspects of confessing (and we might therefore incorporate Goffman's work as well). Jefferson has made a provocative comment relevant to this discussion:

Thus, we might be studying a culture which has gotten control of small interactional units, but is not yet able to properly cope with large units. Such an image projects an evolved-to future in which the proper positioning of some Nth component of an occasionally- activated large package is insisted upon to any child and lapses thereof are complained of by any coparticipant. Conversely, we might be seeing, in the ritualized elements, remnants of a more primitive, rigid version of interaction, from which the culture has been in the process of devolving as it becomes more interactionally sophisticated. The projection, then, is of a future in which it is no bother or issue as to whether someone returns a greeting, answers a question, now, later, or at all; where, upon the occurrence of such a strictly ordered pair, a participant might remark upon the coincidence that just last week something similar occurred. (1988: 439, fn. 6)

Jefferson's first future option does indeed lend itself to a confession model: children socialised to recognise and respond to conversational packages which require confession. But, the second option is quite contrary to a confession model, as it suggests that modern Western societies may be becoming less and less confessional in nature. Obviously, this matter requires further detailed study.

Such matters of enquiry are theoretically interesting and indeed worthy of further study. However, as I hope to have shown in this chapter, there are good grounds for believing that if sociologists wish to study disclosure in this way, then they should include consideration of the organised nature of the talk within which disclosures are accomplished. Without analysis at this level, these lines of enquiry will be, at best, theoretically interesting, but at worst, quite unrelated to everyday activities.

Chapter Three

The Language of Reproduction: Is it Doctored?

3.1 *Introduction*

Feminists have made a significant contribution to the sociological literature on infertility. This ranges from work with a broader theoretical relevance to infertility, such as O'Brien's *The Politics of Reproduction* (1981), to work more specifically concerned with women's experience of infertility (eg. Lorber, 1989; Lorber & Bandlamudi, 1993; Pfeffer & Woollett, 1983; Sandelowski, 1990a, 1990b; Unruh & McGrath, 1985). In addition, there is now a large body of feminist research on the new reproductive technologies (eg. Arditti, Duelli Klein & Minden, 1984; Corea, 1985; Klein, 1989; Overall, 1987; Spallone, 1989; Spallone & Steinberg, 1987). The vast majority of this latter research is very critical of the way contemporary medical technology is applied in infertility diagnosis and treatment. The extent to which this critical element has grown, is shown by the formation of an international network of feminists opposed to the new reproductive technologies (FINRRAGE¹), and the establishment of a journal devoted to critical analyses of reproductive medicine (*Journal of Reproductive and Genetic Engineering*, established 1988).

¹ Feminist International Network Resisting Reproductive and Genetic Engineering

The size of the latter body of literature suggests that the dominant position amongst feminists is to approach infertility as a subset of the analysis of the oppression of women by men. Certainly, it is conventional for feminists to be opposed to the new reproductive technologies. However, some feminist scholars have been prepared to question the dominant view, arguing, in essence, that it is too deterministic and inflexible. Stanworth (1987) has singled out O'Brien's (1981) argument that men have tried to make up for procreative discontinuity by dominating nature and women, as an example of this overdeterminism. Gerson (1989) suggests that feminists have neglected to account for the way in which many women actively seek out reproductive technology and male medical specialists; and similarly, Sandelowski (1991) has criticised pronatalist and patriarchal explanations of the "never-enough quality of conceptive technology". The overall tone of these arguments is well captured by St. Peter's statement that

... we make a serious mistake if we assume feminism must oppose itself theoretically and absolutely to reproductive technologies ... the new radical feminist position that treats women as a unified category under threat from a total male technological-medical takeover, "forces a false unification of women's interests" and "comes perilously close to accusing women of false consciousness if they are not adamantly opposed to the nRT's [New Reproductive Technology]". (1989:367)

The work noted above indicates that there is a growing divergence in the way feminists are approaching the 'politics of reproduction'. Amongst the divergence, there is a common recognition though, that the issues being discussed are serious ones.² Questions about the

² Strickler (1992) presents a concise discussion of the divergent interests of the medical fraternity, infertile consumers, and feminist critics, as related to the new reproductive technologies. She makes a good case that these groups have

status of parenting and the division of labour in that task, what medical practitioners, scientists and technicians should and should not do to assist people to conceive, where assistance ends and experimentation begins, whether there really is a need for expensive infertility treatment given current world population growth, whether medical technologies objectify women's and men's bodies, are all important issues. However tempting it is to make some direct comment on these issues, this option will be avoided. Instead, discussion will be focused on a very select area: amongst feminist approaches is a specific argument which suggests, putting it simply, that the language of reproduction is doctored to suit the interests of both, male medical practitioners, and, men in general. I think this argument is seriously flawed, and in the first part of this chapter a critique is offered of some important examples of feminist arguments about the language used in describing male (in)fertility or the male reproductive system.

In the second part of this chapter, an ethnomethodological analysis of a piece of medical discourse is presented. This analysis is very different to the feminist analyses of the language of reproduction. This difference stems from the distinct aims of ethnomethodology and feminism, and I would emphasise here that the analysis is not presented to suggest that one aim is superior to the other. That said, in this case it will be argued that the general principles of an ethnomethodological approach to textual discourse have important implications for the feminist arguments about the doctored language

of reproduction. The supposed 'doctored' nature of medical discourse is now outlined in the first section of this chapter.

3.2 *The Doctored Language of Reproduction*

As indicated above, there is a considerable amount of feminist literature relevant to the topic of infertility. However, this literature is broad ranging, and there is actually very little which focuses specifically on the language used in describing male (in)fertility or the male reproductive system. Consequently, only three examples of feminist arguments about the language of reproduction will be discussed here. This is sufficient given that the aim is not to make a general critique of feminism, but to focus on specific arguments which relate to the topic of male infertility.³

Pfeffer's (1985) paper, "The hidden pathology of the male reproductive system", is, as far as I know, the most detailed feminist treatment of medical description of the male reproductive system. She begins her argument with difference, that is, women and men have different health experiences. Women visit doctors more frequently than men and this is especially so for conditions associated with reproduction. Pfeffer notes that 'biology' is a "well rehearsed" (30) explanation of this difference: the female reproductive system has more functions and is therefore more structurally vulnerable to problems than the male reproductive system, which operates with less

³ Extensive quotes will be used in this section in order to give a good indication of the arguments being made. Undated references will refer to the respective work under discussion, and where quotations include emphasis this has been reproduced from the original, unless stated otherwise.

problems. In reaction to this view, many feminists have stressed that biology itself is a 'social construction': the "definition of women as biologically vulnerable and unhealthy is a social construction formulated in male terms, instrumental in the subordination of women by men" (31). Medicine has defined normal female bodily processes as pathological, that is, they have been "medicalised". In tandem with this process medicine has also trivialised or overlooked legitimate women's complaints associated with reproduction (eg. pre-menstrual tension).

Pfeffer sides with the feminist view, but suggests that there is something of a quandary here: just what is, or is not, a legitimate female reproductive disorder? "What medicine describes as abnormal, feminists claim is 'normal', and vice versa" (31). Pfeffer argues that this contradiction arises because feminists have fought only half the battle: medicine is instrumental in the subordination of women by defining them as weak, but feminists have neglected consideration of the way medicine also defines men as strong, hence contributing to their powerful position in society. On this basis, Pfeffer sets out to challenge the assumption that the male reproductive system is structurally efficient:

I will argue that whilst medicine highlights the potential for reproductive disorders in women, it makes them invisible in men. As Ruth Hubbard suggests 'we need to construct a more inclusive and, in that sense, truer reality by pulling forth facts that have previously been ignored, while pushing back others which have received more notoriety than their substance merits' (Hubbard, 1981, p. 217)
(31-32)

After this formulation of her research problem, Pfeffer indicates what data she will be dealing with: "I have drawn on medical sources and those of reproductive physiology which inform much of medical knowledge" (32). To indicate that her approach to this data is theoretically informed, she then makes positive reference to Dale Spender's argument in *Man Made Language* (1980) that language enhances the male image while diminishing the female image. Pfeffer effectively summarises her analysis by stating that "The language of reproductive medicine is no exception to this finding" (32).

The overall tenor of Pfeffer's argument, and her analytic method, can both be seen in the following quote from the early stages of her paper:

There is a whole host of negative words in medicine to describe the female reproductive system. A women can suffer from *irregular* menstrual cycles caused by hormonal *imbalances* which can lead to *hostile* cervical mucus and *irregular* shedding of the lining of her uterus which may be structurally *retroverted*. If she does ovulate, she may fail to conceive because of *blocked* fallopian tubes. Or she may conceive an *ectopic* (*wrongly placed*) pregnancy. Then she faces the danger of *spontaneous* or even *habitual* miscarriage due perhaps to a *blighted* ovum or even an *incompetent* cervix. And once she has given birth, her uterus may suffer *chronic subinvolution* and subsequently *prolapse*. The picture created is of a precarious, inefficient system.

For men, there are almost no negative terms; testicles may be undescended and the prostate may become inflamed or enlarged but the vocabulary which so negatively describes women is not available for men. (32-33)

In short, there are far more negative terms applied to the female reproductive system, than to the male system. Pfeffer follows this by arguing that discussion of the male reproductive system moves away from the organs themselves and focuses on the quality of semen.

Again, supposedly, we find the same gender bias operating: the health of the ovum is rarely discussed except to impute blame, whereas only positive terms are used to discuss the quality of semen. Semen is classified by the motility and shape of sperm - in simple terms sperm can have normal or abnormal shape and motility. According to Pfeffer, the mode of classification is positively biased because "the measures of fertility are expressed in positive terms [ie. 60 % normally shaped, 60% motile] and not as 40 per cent misshapen or 40 per cent immotile sperm" (33).

Pfeffer's argument covers much more, but this is the central piece that deals with language and descriptions of male (in)fertility. Therefore, discussion can now move to consider Beagan's "Jargon, myth and fetishes: Language use and the new reproductive technologies" (1989), and Steinberg's "The depersonalisation of women through the administration of '*in vitro*' fertilisation" (1990).

In a similar manner to Pfeffer, Beagan cites Spender's work as a justification for claiming that language constitutes a mechanism of power. She does note that the relationship between language and action is complex (too complex to be pursued within the limits of her analysis), nevertheless, she is confident that "to the extent that it illustrates or affects the "reality" of medical professionals, the language use examined here indicates a serious threat to women" (4).

Beagan's analysis consists of an "informal content analysis" (4) of articles on new reproductive technologies primarily from the journal *Fertility and Sterility*. Her basic argument is that the language used to describe reproductive technologies reflects the aims and interests of

a male dominated medical science. This language supposedly accomplishes a variety of tasks including: limiting access to information; selling the technologies to the public; justifying and facilitating experimentation on women's bodies; dehumanising women; and constituting women as reproductive raw materials (4). She claims that

Some of these political/linguistic functions are clearly intentional; others are incidental to the usual processes of science. Regardless of intent, these linguistic devices can prove an extremely effective means of expressing or instilling values - in this case, clearly misogynist values. (4)

The majority of Beagan's paper then presents snippets of the language of the new reproductive technologies accompanied by a rendition of the function of this language. Included is a brief mention of the medical description of the male reproductive system, and it is remarkably like Pfeffer's argument:

Sperm, unlike ovaries, ova, or uteri, are given agency. Sperm swim, penetrate, fertilize. Sperm may be categorized by their higher or lower "fertilization ability" (Tanphaichitr, 1987). Ova, in contrast, are categorized by their ability to be fertilized. ... In men, infertility is treated with linguistic kid gloves, termed "male-factor infertility," or "male-related infertility." In fact, when the infertility is "male-related," women's passivity suddenly vanishes, and responsibility is shared equally. Thus, for example, Matson et al., (1986) refer consistently to "oligospermic couples." (6)

In sum, Beagan makes the traditional feminist argument that the new reproductive technologies are the result of a masculinist medical science and the language they use reflects this reality. It is only in the final paragraph that Beagan mentions the women undergoing treatment with new reproductive technologies. She notes that they are not passive recipients nor simple victims of male medical exploitation, nevertheless, they have difficulty in finding their own

'voice'. She concludes by stating that a "Truly accurate language to describe NRTs from the perspective of women is simply not available yet ... the development of language which can describe women's experiences of oppression has just begun" (7).

Consistent with Beagan's concerns, Steinberg offers an analysis of the "depersonalisation" of women through reproductive technologies. Just as Pfeffer began by detailing gender differences in health experience and medical terminology, and Beagan noted the male dominance of medical science, Steinberg also begins with the premise of difference. But Steinberg's view is more comprehensive, including four central premises (74-75):

1. The tools and technologies of medical science are designed, developed, named and utilised by experts who are nearly always men.
2. Medical scientific technologies are never value-free or neutral - they encode the consciousness of those who invent and use them.
3. The medical scientific community is a "white, patriarchal (male dominated) and powerful community" which has been "remarkably free from accountability to anyone but themselves" (75).
4. "Women are globally and differentially subordinated to men, male power and men in power" (75).

On this basis Steinberg presents a lengthy discussion of the depersonalising effects of the new reproductive technologies. She argues that the term 'infertility' establishes a pathological identity, orienting an individual's personhood around 'dysfunction' or 'disease', drawing people labelled 'infertile' firmly into the medical realm:

It is a system or classification which rationalises, justifies and calls for medical intervention. The way in which 'childlessness' is seen as a physiological 'dysfunction' locates it firmly (and

only) within the domain of medical science. It posits not only the state of 'childlessness', but those individuals who are childless (defined as 'infertile') as appropriately, even necessarily, medical territory. In so doing, the classification 'infertility' sets up a *categorical imperative* for medical intervention. 'Infertility' is set up as a medical problem which, therefore, necessitates a medical 'solution'. (91)

In addition, the category 'infertility' is supposedly "*covertly gendered*" (91). That is, there is a bias underlying descriptions of infertility which situates responsibility for infertility with women. Steinberg provides three factors to elaborate this claim, but it is only the third⁴ that is directly concerned with language. She argues that the way in which patients undergoing infertility investigations are termed 'infertile couples' compounds the gender bias of the term 'infertile'. Referring to couples may appear to avoid apportioning blame, but it does not in fact do so. Rather, it "*subsumes* and therefore *trivialises* women's unequal burden and risk in undergoing 'infertility' investigations under a rhetoric of equivalence" (92).

The other major argument about language that Steinberg makes is little different from Beagan's argument. That is, the language of the new reproductive technologies removes agency from the women and places emphasis on the practitioners and the procedures they use, rather than on the women involved. In this process women's body parts are treated as disembodied fragments (85). However, Steinberg is more sophisticated than Beagan in her theorisation of the relationship between language and activities. The relationship is

⁴ In the first, Steinberg utilises anecdotal evidence from a self-help guide for infertile couples, which supposedly indicates that doctors pay more attention to the sensibilities of infertile men than women. Secondly, she argues that the structure of infertility investigations also displays a gender bias: supposedly, the investigation of women is much more intrusive and intensive than the investigation of men.

supposedly a recursive one: the way the practice and language of the new reproductive technologies erase women reflects a prior understanding of women as depersonalised entities (ie. the male world view outlined in the four premises), but

At the same time, 'IVF' treatment and language concretely (re)construct this depersonalisation of women. The way women are represented and, in turn, positioned and (re)constructed in the context of 'IVF' treatment reveals an entire conceptual framework implicitly concerned with the social meaning of women and the relationship of medical science to women, particularly to women's reproduction. This conceptual framework is not separate from the 'machinery' of 'IVF' but is **implicitly encoded** into its very procedures and tools. (89, emphasis added)

In other words, the language and practices of the reproductive technologies can be read for the patriarchal character of the society in which they occur. Their very possibility is a reflection of patriarchal society, and in turn they further the superordination of men and the subordination of women in patriarchal society.

3.3 *Critique*

In Pfeffer, Beagan and Steinberg's analyses of the language of reproduction there is a common general argument and some shared specific points. The general argument is as follows:

- i) society displays gender inequality whereby men dominate women;
- ii) the institution of medicine is not neutral but shares the character of the society it operates within;
- iii) thus when medicine focuses on reproduction, both its activities and language are gender biased (for men, against women);

- iv) this bias is possible because of i) and in turn contributes to the reproduction of gender inequality in society.

With this general argument as a framework, five specific points are made. First, negative words are used in description and discussion of the female reproductive system, but, in contrast, the male reproductive system is treated with "linguistic kid gloves". Second, medical descriptions of male (in)fertility move quickly away from possible dysfunction to a focus on the quality of semen, which again is described positively. Third, in descriptions of the new reproductive technologies, the practitioners and procedures are given agency, while women disappear from view or are treated as mere body parts. Fourth, the language of reproductive technologies produces pathological identities, also focused on women, which capture the infertile in a system of treatment glorified and euphemised to hide its real exploitative nature. Fifth, there is currently no language in which to authentically voice women's experience of the new reproductive technologies.⁵

In considering these arguments it should be stressed that the following critique is not concerned with the more general feminist proposition that modern societies display gross forms of gender inequality (for men, against women). There is no argument on this score. However, I do take issue with the specific points made in the feminist analysis of the language of reproduction, as the following critique details.

⁵ For the sake of brevity, at times in the following discussion these five points will be referred to as 'point one', 'point two', 'point three', and so on.

In my view, point one is the product of very tendentious reasoning. Leaving aside for a moment the issue of what a "negative" word is, any quick scan through medical literature on infertility will turn up "negative" words referring to the male reproductive system or its functioning. There are more than enough of these words to reject the claim that male (in)fertility is treated with "linguistic kid gloves". Using Pfeffer's italicising device one can concoct a convincing list of woes: men can suffer from *weak* or *short-lived* erections, *retarded* or *retrograde* ejaculation, or indeed, the *absence* of ejaculate. If there are no problems with erection or ejaculation, a man can display genital *underdevelopment* or *soft* and *reduced* testicles, and he may also suffer from endocrine and chromosomal *defects*, or *obstruction* in the vas deferens or epididymis. If his sperm count does not reach the stipulated level he may be described as having *severe* oligospermia, and sperm itself may have various *defects*, *abnormalities*, *immaturities*, or *aberrations*.⁶ For a "negative" description of sperm one could not go far past this piece:

The spermatozoa of many infertile patients often display sluggish, ineffective motility (asthenospermia) or total immotility; this could be the expression of either sperm agony or death (necrospermia), or of defects of the motor apparatus. (Zamboni, 1987: 726)

I have resisted italicising the "negative" words here as given the previously established pattern the reader should know how to interpret this piece. With only this brief consideration, it seems fair to say that the "linguistic kid glove" argument is based on a very selective reading of medical discourse.

⁶ These words were culled from Joel (1971), and Newton (1983) with a minimal amount of effort - there are many other "negative" words used to describe the male reproductive system.

But, looking again at the preceding quote, note how it refers to "infertile patients" and not infertile men. Could this not be evidence for the claim, made as part of point four, that male infertility tends to be subsumed within terms like "infertile couple", thus displaying a double bias against women? The title of the paper from which the quote is taken is "The ultrastructural pathology of the spermatozoon as a cause of infertility: The role of electron microscopy in the evaluation of semen quality". This could be used to further the argument, that is, the title avoids specific mention of male infertility, using instead the couple inclusive term "infertility", thereby concealing male infertility behind the more general category. However, if we approach medical texts with a notion of readerly competence, there are strong grounds to doubt that concealment of any kind is occurring. Readers find the meaning of individual words not in the words themselves but in relations between words and their placement within a sentence, and in turn, sentences gain meaning in relation to prior sentences and prospective sentences. In the sentence quoted above the term "infertile patient" is part of the larger phrase "The spermatozoa of many infertile patients", thus suggesting that the topic is male infertility. The same applies to the paper's title: although the term "infertility" is used instead of specific reference to male infertility it is clear through reference to "spermatozoon" and "semen" that the paper is concerned with an aspect of male infertility.

Similarly, once we introduce a notion of readerly competence, Beagan's argument that the phrase "oligospermic couples" conceals male infertility looks somewhat silly. The paper where she finds the term is in a journal of obstetrics and gynaecology, and we might expect that the readers of such a journal would know the meaning of

"oligospermic" and could ungloss the term "oligospermic couples" as 'couple infertility resulting from male factors, specifically a reduced sperm count'. Lay readers might also deduce from the "spermic" part of the phrase that it refers to male factor infertility, or indeed, they might well consult a glossary of infertility terms to find its meaning. Clearly, the point here is that the argument that the categories used to refer to infertility conceal male pathology and doubly emphasise female pathology, is a 'cultural dope' model of texts and audiences. It hardly needs to be said that such a model is no longer convincing (see McHoul, 1982).

Now consider point two. That is, as Pfeffer argued, the mode of classifying semen is positively biased because "the measures of fertility are expressed in positive terms and not as 40 per cent misshapen or 40 per cent immotile sperm" (33). But the problem here is that if medical practitioners measure the fertility of semen in terms of percentage motile and percentage normally shaped, and Pfeffer measures it in terms of percentage immotile and percentage abnormally shaped, how do we decide who is right? While I am wary of using a 'sledge hammer to crack a nut' it seems that Wittgenstein on language games⁷ is most germane here. For, the best way to approach this problem is actually to drop a notion of 'right' or 'wrong' measurement and focus on presuppositions. Negotiating an agreement on the measurement of semen is not possible because the truth of the matter cannot be gained from negotiation: the medical practitioners and Pfeffer are approaching the measurement of male fertility from different vantage points, different 'language games'.

⁷ The argument in this paragraph is based on a discussion of Wittgenstein by Button and Sharrock (1993).

When medical practitioners use the same measure within medical discourse "agreement resides in the consensus in their activities in a language game" (Button & Sharrock, 1993: 14), and if Pfeffer persisted in measuring fertility by percentage immotile and percentage abnormally shaped, then the medical practitioners would conclude the Pfeffer does not know how to participate in their language game - she had not learnt its basic principles. It is not that the form of medical description is necessarily correct, but that it is what is intelligible, 'naturally' sensible, within the language game of medicine.

Currently, the basic principles of medical knowledge about male fertility are that sperm density, motility, and shape, are important in achieving fertilisation. Following from this, the investigation of male infertility takes the following logical form: firstly, assess the presence of sperm - if there is none, then this is a first categorical cut-off point where further investigations are pursued - then, secondly, evaluate the motility and shape of the sperm in relation to a conception of their 'normal' state - if the sperm fail to reach the stipulated criteria then apply a second categorical cut-off point with further investigations.

Now, Pfeffer can certainly take issue with the presuppositions of the language game of medicine, but this is not what she has done. Her argument is that the pathology of the male reproductive system is **hidden**. This is not an argument about presupposition, it is essentially a functionalist argument. Whether there is any mention of intention or not, she argues that one function of medical descriptions of reproductive systems is to hide the pathology of the male system and emphasise the pathology of the female system. Obviously, this suffers from the traditional problem of functionalist reasoning, in

short, tautology, but I do not want to go into the weaknesses of functionalist argument here (see Giddens, 1984 for a good discussion). The point is, that the form of medical description of male fertility is part of a language game - that is where it receives its sense. Surely, the view that description of male (in)fertility bears the marks of patriarchal society, and functions to reproduce the inequalities of that society, seriously ignores the initial framework within which descriptions of male (in)fertility are situated.

Moreover, it seems to me that the medical description of male fertility reflects a generic, as opposed to gender based, way of doing description. Quite simply, if in any practical activity, there is a concern for an end state and it is known (or presumed) that some entity will accomplish that end state, then the rule is to measure the presence rather than the absence of that entity. With male fertility, given that most males are fertile, and have rates of motility with sperm shape within the 'normal' range, it is good common sense to describe by presence - percentage motile, percentage normally shaped. It is also possible that sperm may have a specific "normative identity" (Lynch, Livingston & Garfinkel, 1983: 224)) whereby description of it cannot escape commonsense knowledge of its 'proper' function.⁸ In her pursuit of a feminist explanation of the description of male fertility, Pfeffer seems to have missed both this point, and the embeddedness of medical descriptions within a language game.

⁸ This comment is made speculatively and will not be taken up in any more detail. See Lynch (1991) for a good discussion of the way 'measurement' can be treated as an ethnomethodological topic. Lynch includes a good discussion of Sacks' notion of "usualness measures" which is clearly relevant to my suggestion that sperm have a "normative identity" - in short, measurement is inescapably a social activity and cannot escape everyday knowledge of what is being described, and what would constitute a 'normal' description.

The neglect of local production

In the remainder of this critique I would like to follow on from the above argument and focus on the way Pfeffer, Beagan, and Steinberg's approach ignores a wide range of factors involved in the production and reception of textual descriptions of reproductive systems and technologies. To begin, it is useful to consider an adaptation of Dorothy Smith's (1974b) model of the production and reading of textual documents:

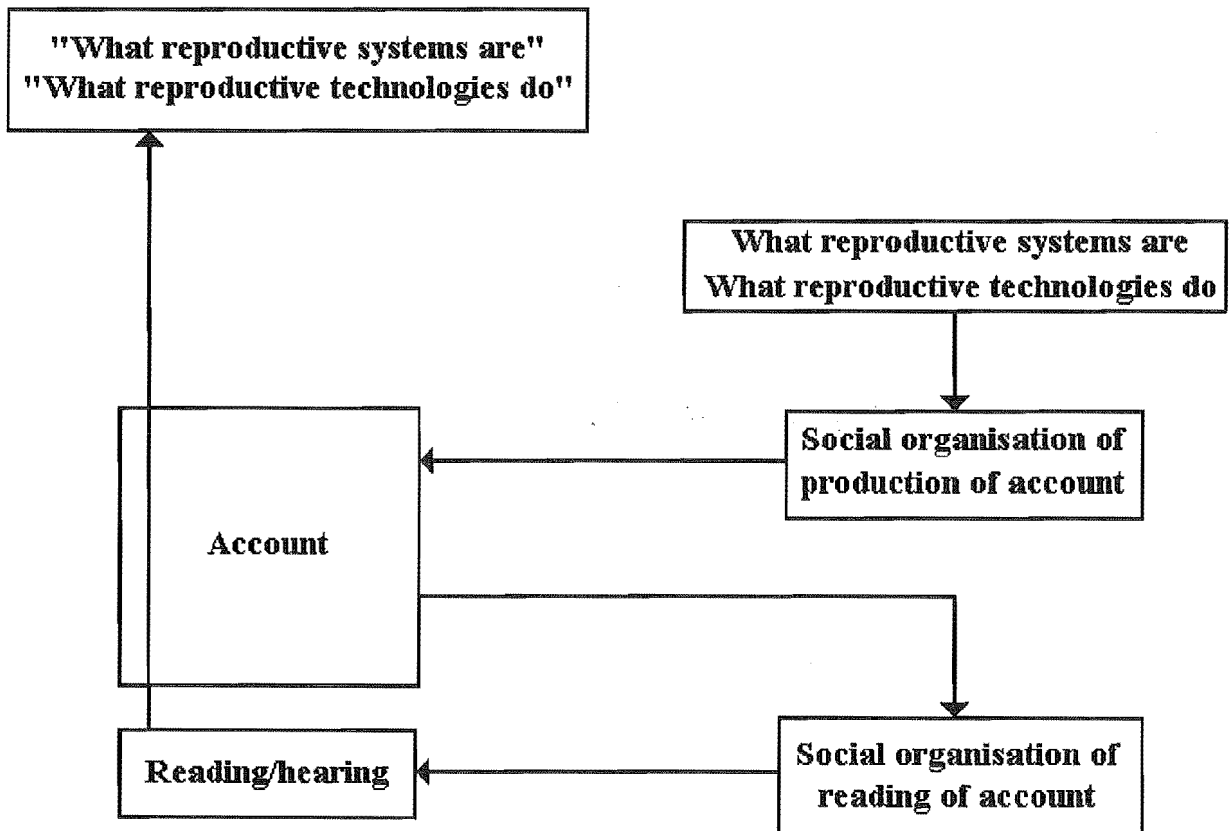


Figure 3.1 The Production and Reading of Textual Documents (Accounts)

In Smith's original model the top two boxes were, respectively, "what actually happened" and, what actually happened (ie. no quotation marks). I have modified Smith's model by substituting "what reproductive systems are"/"what reproductive technologies do" and the same terms without quotation marks.

In the feminist analyses the accounts focused on are those from the medical realm, and clearly, the main point of their analysis is that "what reproductive systems are"/"what reproductive technologies do" is mediated by the social organisation of production of account. This latter input is conceptualised in very broad terms as the gendered nature of society, that is Steinberg's four premises about patriarchal society, and Pfeffer and Beagan's reduced versions of the same, or, on a more specific but still very general level, it is conceptualised as the masculinist medical system or the masculinist new reproductive technologies.

With reference to figure 3.1, two points can be made very quickly. First, if language embodies or imposes a world view, as Pfeffer, Beagan and Steinberg argue, how have they managed to escape it? How has language which functions so repressively allowed them to see through its repressive machinery⁹. While they are certainly

⁹ This 'exemptionalist' point of argument has probably been noted many times. Sharrock and Anderson (1981) provide a good rendition, which helped me realise its importance, and for a cogent critique of social constructionist approaches which bears on this issue, see Woolgar and Pawluch (1985). They question how sociologists can claim that other people's knowledge is socially constructed but avoid applying that finding to their own knowledge - they provide the apt term "ontological gerrymandering" for this process. More relevant to feminist discourse is an interesting paper by Mulkay (1991 (1989)) which I read as a parody of

correct in maintaining that ideas (and medical descriptions) are historically contingent and hence 'socially constructed', Pfeffer Beagan and Steinberg cannot get away from issues of reference: underlying their concern with biased description is a concern with 'correct' description. But as Wittgenstein and many others have noted, there is no possibility of 'correct' description: language is not a nomenclature, there is no neutral 'reality'-mapping-process available to us (see Sacks, 1963). The upshot of this is that some languages cannot be more authentic than others, and there is little sense in appealing to the private experiences of women (or men) undergoing treatment by the new reproductive technologies in order to pursue a notion of authenticity. Subjects who speak of medical experiences cannot utilise a private language, rather they must use publicly available ways of speaking (Silverman, 1987). Thus, we might wonder just what Beagan is calling for when she notes that a "Truly accurate language to describe NRTs from the perspective of women is simply not available yet" (7). We might also wonder about Pfeffer's supportive quoting of Hubbard: "we need to construct a more inclusive and, in that sense, truer reality by pulling forth facts that have previously been ignored, while pushing back others" (quoted above). Just who will play the role of neutral arbiter to select which facts need to be pulled forth, and which need to be pushed back - *quis custodiet ipsos custodes?*

Second, Pfeffer, Beagan and Steinberg totally neglect the bottom part of the model - the social organisation of the reading of account and the reading/hearing. To a large degree this is what enables them

contemporary claims that the "Women's Movement alone offered the possibility of rebuilding social life on a new linguistic basis" (215).

to argue, from a few snippets of the language of reproduction, that the infertile are trapped in a system that pathologises their identity, that limits their access to information, that justifies experimentation on their bodies, that hides male pathology and emphasises female pathology, and so on. To recall a quote from the introduction to this chapter, such a view comes close to accusing women of false consciousness if they are not opposed to the new reproductive technologies, but more than this it seems to doubt whether they have any consciousness at all! Just how does the label 'infertile' set up a "categorical imperative" (Steinberg) that draws one irrevocably into the clutches of the "technodocs" and their experimental medical technologies? Is the label actually used in infertility treatment and by whom? More specifically on male infertility, who is it that reads the medical textbooks that are supposedly hiding the pathology of the male reproductive system, and with what purpose do they read them? Language does nothing by itself, but this simple point seems to have eluded Pfeffer, Beagan and Steinberg who have gained more mileage from treating the readers and writers of medical texts as 'cultural dopes'.

Despite the undoubted cogency of the claims about gender inequality in society, it is simply not sufficient to present some decontextualised fragments of language in the light of gender inequality and then claim that the function and meaning of such language is clear. In the absence of detailed knowledge of the whole range of activities represented in figure 3.1, the feminist accounts of the language of reproduction bear an unknown relationship to the activities that they depict. The language of reproduction, just like any language, is a "naturally organised ordinary activity" which cannot be

dissociated from local organisational practical purposes. As Garfinkel and ethnomethodologists have well established, the meaning of language is not derived from pre-established culture in a top-down manner, rather, meaning is established in a multitude of shared methods of reasoning which are particularised to **local circumstances** (Goodwin & Heritage, 1990). In contrast, Pfeffer, Beagan and Steinberg posit a "bucket" theory of context, in which the situation of action is treated as anterior to - as "enfolding" and determining - the action that takes place within it" (Goodwin & Heritage, 1990: 286). In their case the "situation of action" is patriarchal society, or nested versions of it.

This view has a strong bearing on the argument about "negative" description of the female reproductive system and "positive" description of the male system. The ethnomethodological view is that it is not for analysts to simply state what the meaning or function of language is. Rather, they should attempt to show how the participants to social action enact and display the meaning of their language, in the process of using it. It is faulty analytic practice to assert that a word or phrase is "negative" without considering the fullness of language-in-use. Consider the following extract of talk:

A: Fuck you

B: Man, you haven't even kissed me yet.

(Goffman, 1971: 216, quoted in Manning, 1989: 368)

It would not be hard to argue that the term 'fuck you' has a "negative" meaning, and commonly so. But this extract shows that the possible "negative" meaning can be reframed by a recipient's rejoinder. How the term should be categorised after B's reply would require some discussion, but it certainly seems far from a simple "negative" term.

Exactly the same process can occur with what seem like unproblematically "positive" words, as the following example of an encounter between a black doctor (D) and a white policeman (P) shows:

P: 'What's your name, boy?'

D: 'Dr Poussaint. I'm a physician.'

P: 'What's your first name, boy?'

D: 'Alvin.'

(Speier, 1973, quoted in Watson, 1984: 63)

Here, the "positivity" of the words 'Dr' and 'physician' are rendered inoperative by the white policeman, who insists on treating the black doctor as a 'boy'.¹⁰

These examples should make clear how serious the feminist's omission of the social organisation of reading of account and reading/hearing is. Of course, the above examples are extracts of talk and not texts, but there is a developing body of work that provides good grounds for believing that similar principles apply to language in texts (eg. Anderson, 1978; Anderson & Sharrock, 1979; Crook, 1989; McHoul, 1982; Morrison, 1981). Regardless of the problem of comparing talk and text it is still the case that texts are read by competent members thus making reading a social accomplishment. The upshot is that the meaning of words cannot be unproblematically fixed by analysts.

¹⁰ Who uses a word is also important for its situated meaning. Watson (1983: 36) notes the following about "nigger":

... if someone who is categorised as "(a) white" terms some other person as a "nigger," then that term may readily be seen as a "put-down." However, if the speaker is not white but is also categorized as a black person, and he calls some other black person a "nigger" then the term may not necessarily be a "put-down." In some local ghetto cultures in the USA, young male blacks may call each other "nigger" as an upranking rather than a downranking category label....

To hammer this point home, reconsider how Beagan makes the argument that women are depersonalised in the language of reproduction, while medical practitioners and their procedures are foregrounded. In considering medical journal accounts of the new reproductive technologies Beagan claims that

Agency is attributed almost entirely to the (predominantly male) technodocs: they do the stimulating, the inducing, the fertilizing, the transplanting, and the delivering. Consider the attribution of agency in this article abstract: "After *performing* 56 nonsurgical uterine lavages in 42 fertile donor women, we *transferred* 17 conceptuses and *produced* eight pregnancies. Four of the infertile recipients have now *been delivered of* healthy neonates" (Formigli et al., 1987, my emphasis). In patriarchal accounts, body parts, women's stand-ins, passively receive the administrations of male doctor-scientists, the central actors in the script. (1989: 6)

With a little help from the familiar italicising device, Beagan's suggestion that this abstract illustrates the dominance of "technodocs" accomplishments seems feasible.

However, even technodocs' accomplishments are not made solely in abstracts. From looking at the journal that this abstract came from - *Fertility and Sterility* - it was very apparent that Beagan's argument misrepresents the local production site within which this abstract is located. The paper the "abstract" summarises is part of the "Communications-in-brief" section of the journal, and as such an "abstract" is not included with the text. What Beagan calls an "abstract", actually comes from the contents page of the journal where all papers in the issue are summarised under their title. In the issue we are concerned with there are twenty eight papers listed and the "abstracts" generally take the form of a one or two sentence statement of the issue examined and results. In this local context the abstract

that Beagan chooses is very atypical: it is the sole abstract that uses the pronoun "we". The remainder all avoid the use of personal pronouns and utilise a style where X "is reviewed", Y "is supported", and Z "is evaluated". Here are two typical examples:

The use of frozen semen in artificial insemination is strongly recommended over fresh semen. Its comparable fertilizing capacity and superiority in testing for AIDS is supported.

The increment in serum progesterone after vaginal administration during the luteal phase was inversely related to duration of treatment. Possible mechanisms for this observation are discussed. (both from contents page, *Fertility and Sterility*; 1987; 47(1))

When reading twenty seven other "abstracts" like those above, it is not the italicised portions of Beagan's "abstract" that stand out, but the use of "we".¹¹ It is the "we" that is the pivot for the active form the abstract takes (eg. "performing", "transferred", "produced") and without it Beagan could not claim that the "male doctor-scientists" appear as "the central actors in the script". But as I mentioned above, this is a very atypical "abstract"; it would have been far more consistent for Beagan to have argued that medical discourse avoids pronouns because this furthers the "technodocs'" ability to conceal their involvement in their suspect medical practices.

The latter tack is actually the dominant tenor of Pfeffer, Beagan, and Steinberg's arguments. Either way, the point being emphasised here is that it is dangerous to neglect the local practical purposes

¹¹ When browsing through other contents pages of *Fertility and Sterility* an abstract that began "A young woman with a small prolactinoma ..." (1992; 58(2)) stood out in a similar way to Beagan's abstract. In this case the paper is based on a single case study and amidst the large sample, third person style of other papers, the reference to "A young woman" marked it for notice.

involved in the production and reading of texts. Abstracts, for example, are very specialised pieces of writing, therefore, any consideration of pronouns or other discursive characteristics used in abstracts surely must take into account how the discourse accomplishes 'abstracting'. Equally, the feminist approach forgets that medical journals are designed for a specialist audience, and together, audience and journal discourse represent an existential world undergirded by background knowledge. Gynaecologists, obstetricians, scientists, nurses, and so on, who read journals like *Fertility and Sterility* are perfectly aware that women give birth, that it takes effort, that it is painful, and so on. It can also be suggested that such people read descriptions of body parts as 'doing specialised description' and not as 'doing depersonalisation'. No doubt when they move from reading texts to examining patients they are quite aware that they have a whole person in front of them. There is no need to pursue this further, it can merely be noted that there is a good amount of literature which suggests that despite medical discourse being technical and specialised, its use within interaction is subject to the contingencies of mundane reasoning and social-organisational procedures (eg. see Cicourel, 1986, 1987; Garfinkel, 1967a; Raffel, 1979; West, 1984).

In sum, I believe that the argument that the language of reproduction is doctored to further the subordination of women and the superordination of men is seriously flawed. The language of reproduction is doctored, but not in the sense of being an ideological tool. It is doctored in the sense that it is produced by and for doctors and it assumes and enacts the background knowledge of this existential world.

If ideology is to be found here, it seems to be more located in Pfeffer, Beagan and Steinberg's theorising rather than medical discourse. Modifying Smith (1974a), their theorising is ideological because it takes the following path:

1. Take some texts on a common theme.
2. Detach pieces from the texts, do not indicate how the pieces were chosen, treat them as data.
3. Manipulate the data to show an order among them.
4. The original texts are now changed into the sociologist's structural forces. The text's practical work and site of production is now turned into a reflection of structural forces beyond it, that is, the structural forces are presented as causing the text.

To put it simply, Pfeffer, Beagan and Steinberg have taken medical discourse and found what they wanted in them, rather than treating them as one aspect in a multiple accomplishment of readers, writers and organisational frameworks within existential worlds.

Finally, it is worth noting that amidst all the complaints about the language of reproduction, Pfeffer, Beagan and Steinberg have neglected to suggest an alternative lexicon for the description of reproductive systems. Just what words do they propose to use to replace the supposedly doctored language of reproduction? Perhaps the trouble here is that we are back to the search for an authentic language, and given that such a language will never be found, any existing language-in-use can be found wanting, that is, displaying the marks and purpose of "patriarchal society".

3.4 *Analysis: Editorialising on a doctor's issue*

The second part of this chapter is devoted to an empirical analysis of an editorial from a medical journal. The form of analysis is very different to that of Pfeffer, Beagan, and Steinberg. While the extent of this difference almost means the analysis is unrelated to the first part of this chapter, the difference is also the reason why the analysis is included here. This will become clearer below, but for now it can be noted that a principle function of the analysis is to show that along with the critique there is a "reasoned alternative" (see Silverman, 1989, rule 1). Firstly, some details will be provided of how the editorial was chosen as data, along with a brief background of the journal in which it appears. After displaying the full editorial the analysis focuses on the issue of how the text accomplishes the activity of editorialising. A summary of the chapter and an overall implication will be made in the concluding discussion.

Choosing the text to be analysed.

It is important to provide some details of how I chose this particular text as it saves me from my reproducing a fault I noted above, that is, no details are given about how or why data extracts are chosen. Initially, in working up this chapter, I was aiming to find some medical texts that 'disproved' at least Pfeffer's thesis about the lack of negative words used to describe the male reproductive system. An early idea about where to start was provided by a radio report about a medical researcher who had just published a paper in the

British Medical Journal providing evidence for a worldwide decrease in the quality of semen (Carlsen, Giwercman, Keiding & Skakkebaek, 1992). Armed with this possibility and a few others I set out to the medical library at the Christchurch School of Medicine. I located the article on semen quality, and a few others, and began considering their usefulness for the task of 'disproving' Pfeffer's thesis.

In the midst of this process I did some browsing through general pathology texts and medical journals. Going through some current issues I picked up the latest issue of *Human Reproduction*, a relatively new journal that I was not familiar with. Flicking through the journal I had an 'aha' experience upon sighting an editorial with the title "The fraud conviction of Cecil B. Jacobson". I was previously acquainted with the events to which the editorial referred (from newspapers and television news), and a quick glance at it revealed that it was an interesting text. Cutting a long story short, I photocopied the editorial and kept it 'on file' as the first part of this chapter took shape. Subsequently, I developed an analysis of it, but in the process all thought of it being a direct rebuttal of the feminist arguments disappeared, and it became a stand-alone ethnomethodological analysis.

Briefly, before considering the editorial there are three background details worth noting.

1. **The Journal.** *Human Reproduction* was first published in 1986 and is published monthly for the European Society of Human Reproduction and Embryology (ESHRE). This organisation was founded in 1985 as a scholarly forum for European scientists and

medical practitioners working in the field of human reproduction and relevant animal studies. The journal's scope, as outlined in its instructions to authors, includes: "fertilization; early embryology, implantation; pregnancy (to the end of first trimester)". Thus, readers of the journal are likely to include: gynaecologists, general practitioners, urologists, andrologists, medical and laboratory assistants, nurses, endocrinologists, embryologists, comparative physiologists and research scientists.

2. **The slot in the journal.** The editorial occupies the first substantive section of the journal - a section given over to "news and views". This section more commonly presents an "Opinion", although occasionally neither an opinion or editorial are presented. Opinions are usually longer than editorials, and can involve substantial presentation of research findings. In contrast Editorials do not present empirical research but reserve themselves for short comment on relevant issues.

3. **Editors.** Editorial writers come from four groups: the principal editor (Prof. R.G. Edwards); associate editors; international editors; and invited editors. In our case the editorial is written by the principal editor along with an invited editor - Prof. D.J. Sharpe. In the twenty issues that I looked at, this was the only time that the principal editor had written an editorial. This point is mentionable because of the status of the editor: Professor R.G Edwards was a main force in the establishment of ESHRE (Burfoot, 1990), but further, he is the scientific pioneer of in vitro fertilisation. He is undoubtedly an internationally recognised research scientist in the field of human

reproduction, and it would not be an overstatement to say that he is the most well known scientist in the field of in vitro fertilisation.¹²

With these background details the following points can be made. First, the editorial is unavoidably an event in the operations of an organisation. The journal is published for ESHRE (this is stated on the cover) and the principal editor was instrumental in the foundation of this organisation. Hence, it is manifest that the editorial is concerned to speak with ESHRE's practical purposes in mind. Second, while there is no doubt that the text itself provides biographical details of the authors of the editorial, I think in this case background knowledge of Professor R.G. Edwards is brought into action to establish "Author Authority" (Anderson, 1978). That is, the usual reader of the journal will know something of Edwards and his status and this will form part of the background knowledge used in reading the editorial. Thirdly, we might take from the historical course of the journal's issues that "Opinions" are more common than "Editorials" and certainly that editorials by the principal editor are uncommon. This leads to the reasonable suggestion that the editorial has a formal-historical noticeability irrespective of its content.

The Text

The full text is photocopied (slightly reduced) with line numbers added on the next two pages.

¹² Edwards retired from his Professorship in 1991 and the January issue of *Human Reproduction* that year carries a preface, including photograph, curriculum vitae and two personal appreciations, to mark his retirement. One of the personal appreciations is titled, "In the beginning there was Bob" (p. 5) and 'catchiness' aside, this indicates the esteem in which he is held. Also see Burfoot (1990) for a citation analysis of the published work of Edwards, which amongst other things testifies to his importance in the field.

The fraud conviction of Cecil B. Jacobson

The trial of a legal case of concern to everyone practising assisted human reproduction has recently concluded in the USA. On March 4, 1992 a federal court jury in Alexandria, Virginia, found Cecil B. Jacobson, MD, guilty on 46 counts of fraud and six counts of perjury. He was alleged to have told perhaps 20 women patients that they were pregnant, based on levels of HCG in plasma which could have arisen from an injection, and to have used his own semen for donor insemination in perhaps 75 women while telling them he used semen from anonymous donors. On May 8, he was sentenced to 5 years in prison without parole, after his routine motion for a new trial on fraud counts was denied on April 3. He was fined \$116 000 in addition, and the judge commented how he 'had not seen a case where there had been this degree of emotional anguish and psychological trauma'. His appeal is pending.

Conducting the case in a federal court for fraud was a matter of first impression. In the United States, state courts usually handle criminal fraud cases under state law, and state agencies handle abuses of medical licences. As for civil actions for damages, Dr Jacobson's medical malpractice insurance company had already paid former patients large sums of money to settle their claims, and it is no crime, state or federal, to be sued for medical malpractice.

In the early 1970s Jacobson built a worldwide reputation as an innovator in human infertility. His work was not done in a hospital setting, so institutional oversight was absent. Witnesses testified how he told a patient that she had miscarried without expelling tissue and had accompanied his explanation of 'resorption' with instructions not to have a dilatation and curettage nor to consult another physician. Jacobson's public troubles began in January 1988 with broadcasts by a Washington DC investigative reporter, Lea Thompson. Her television series was based upon patients' accounts of Dr Jacobson's HCG treatment, pregnancy, miscarriage and resorption, followed by specialists' contradictions. A local gynaecologist filed a complaint with the Virginia State Board of Medicine in November 1987, having examined two women the same day and finding neither pregnant after Dr Jacobson had told them that they were pregnant. In early 1989, the Board assessed a \$7000 fine after he promised to cease practice for 5 years. After refunding \$250 000 to ~100 patients, Dr Jacobson closed the clinic in Virginia and moved to Utah, where he conducted privately funded genetics research.

The local United States Attorney initiated federal prosecution of Dr Jacobson in 1989. No federal statute makes a crime of impregnating a patient with the doctor's spermatozoa and without

the patient's consent, nor is it a federal crime to tell a patient that she is pregnant when she is not. However, if Dr Jacobson obtained money from patients by lying about whether women patients were, or had been pregnant, or that semen used for artificial insemination came from anonymous donors, the lies could be charged as fraud under federal statutes. The prosecutor accumulated evidence to support federal fraud theories from 1989. Two years later a federal grand jury approved an 86-page indictment of Dr Jacobson for federal crimes of 'wire fraud' in using the telephone to make medical appointments and discuss treatment, 'mail fraud' in obtaining supplies and sending bills, 'travel fraud' because patients crossed state lines to reach the clinic in Virginia, and perjury in 1988, testimony given under oath before the Federal Trade Commission in an action relating to hormone injection treatments.

At the trial in February 1992, the prosecution presented 11 parents as witnesses. Some former patients testified in disguise and under false names to protect 15 children proven by DNA tests to have been fathered by Dr Jacobson, whose resemblance to the children was noted by the witnesses. Employees testified that no anonymous sperm donors ever came to the clinic, though patients stated that the donors were supposed to be medical students or other men who bore a resemblance to the parents. Expert witnesses gave evidence for both sides. Dr Jacobson testified at length about using HCG in exploring his theories of pregnancy. He admitted making mistakes in interpreting sonograms, but denied lying to patients in order to receive money. He was found guilty on all counts by the jury of eight women and four men.

Motive and motivation kept appearing in the case. Some of Dr Jacobson's patients were angry at the government's motivation in bringing the criminal prosecution, which intruded into their privacy. Why did a distinguished medical researcher, married for 30 years to a loyal wife, a religious man with seven children in the family, expose himself to disgrace, loss of licence to practise medicine, and conviction of crime? The jury foreman believed motive was not part of the jury's analysis; he thought that Dr Jacobson's fees were low, and ego was his principal motivation. An ex-patient thought Jacobson's motivation had been scientific discovery, and resented having been used as a guinea-pig.

Under the judge's instructions, the jury could have concluded that Dr Jacobson's conduct constituted criminal fraud, even if he was motivated by ego or scientific exploration. Alternatively, concluding that Dr Jacobson's other motives displaced the intent to defraud, the jury could have found him not guilty, and that would have been the end of the case. The verdict was guilty. The principal ground for appeal is expected to be that the evidence was insufficient to show fraudulent intent beyond reasonable doubt.

Could similar events happen in clinics elsewhere? It is easy

Editorial by: Professor D.J. Sharpe, The George Washington University, The National Law Center, Washington, DC 20052, USA and Professor R.G. Edwards, Extraordinary Fellow, Churchill College, Cambridge, UK

97 to assume they could not, but detailed inspection and verification
98 of records is perhaps the only safeguard. Pressures on doctors
99 to obtain good success rates in assisted reproduction are
100 sometimes enormous, indeed, the fate of a clinic can depend on
101 them. This is not a healthy situation. Clinics and practitioners
102 are licensed under parliamentary legislation in several countries
103 and it is to be hoped that this practice spreads wider. With or
104 without legislation, however, standards must be established
105 nationally—even internationally—and strong steps taken to ensure
106 that the practice of assisted human reproduction is conducted
107 properly.

Accomplishing the Editorial

Whatever else we might see in this text, it is directly observable as an editorial. The position, boldness and size of the caption indicate this in no uncertain terms. Therefore, my principle aim will be to analyse how the text accomplishes the work of editorialising, and this will be undertaken by studying the formal features of the text.

Anderson (1978) has argued that texts display three formal features - sequencing, categorisation, and recipient design - which all point to reading as a "social activity constrained by expectations and concessions" (134). Moreover, these three formal features are

... not optional, but necessary aspects of natural language descriptions. In the very general sense that actors must have names, reports must start and end and readers know what is expected of them, these features are *ubiquitous*. (Anderson, 1978: 134, original emphasis)

Thus, reading the text as an editorial is a social activity based upon the orderly and sequential nature of its discursive contents.

The initial ordering device of the text is the caption EDITORIAL. Familiarity with editorials would inform anyone that the text as an editorial will probably do two main things. For one, it should establish a "mentionable" (Schegloff & Sacks, 1974) to go to work on, that is, a topical issue to pass comment on. But secondly, editorials do not merely describe mentionables - they make something of them. Warning, moralising, worrying, exalting, contemplating, collaborating, taking heed, and so on, are all possible ways to do this. While not downplaying this variety, I will use the shorthand term 'moral' to describe the cultural object editorials produced with

mentionables. This is used in the sense of the saying 'what's the moral of the story?', and it seems clear that a major task accomplished in editorials is making a transition from mentionable to moral. Further, a moral should apply to some group, it should be particularly relevant to some sort of people, or peoples' activity, and therefore if the editorial is to be recognisable as such it should also accomplish the identification of a target population. Part of the second task an editorial accomplishes then, is not only to construct a moral but to make clear its applicability.

The accomplishment of these tasks begins very early in the text. As noted, the caption EDITORIAL could be seen as an instruction to the reader to look for a moral in the text. Then the title "The fraud conviction of Cecil B. Jacobson" immediately provides the particular mentionable to be worked on in transition to the moral. Interestingly, the very next thing that is established is the target population - "everyone practising assisted human reproduction" (ll. 1-2). Thus, the structure up to sentence one is: caption (providing an overall instruction to read for editorialising work), followed by title (providing first encounter with mentionable), followed by explication of target population. I would suggest that this initial structure is very important in setting up a successful transition from mentionable to moral. Detailing the target population before the moral¹³ is one way of indicating that a moral is coming, hence, the details of the

¹³ There is an obvious parallel here to story prefaces. As noted in chapter two, a story preface functions to let the recipient know that an extended turn at talk is coming and to get that turn on its way. In a similar manner, by giving the target population first, the editorial efficiently keeps the reader on track to the moral which is yet to come. There are undoubtedly similar examples in other discursive realms. For example, newspaper advertisements of the form,

'Single, healthy, aged from 18 to 25, and qualified to enter university? Yes.

Have you considered a career in the airforce'
seem to work with the same device.

mentionable that will be provided are framed within a concern for the forthcoming moral. Detailed explication of Jacobson's fraud conviction is never understood as a simple description of legal proceedings against a doctor, but is available for the making of a moral.

Paragraph one follows from the title by presenting details of the activities that Jacobson was alleged to have committed, and again it contributes to the accomplishment of the transition from mentionable to moral. In effect, the paragraph presents a concise summary of the mentionable, but the text does not then proceed from this summary straight to the moral. It seems that in the very fact of having presented a summary and various details, it sets up the relevance of further comment and unpackaging. This is made clear if we summarise the actors and concomitant activities that are presented in paragraph one, as in Table 3.1.

Table 3.1
Actors and Their Activities as Presented
in the First Paragraph of the Editorial

Actors	Activities
everyone practising assisted human reproduction	concern about the case
federal court jury	deliver guilty verdict
Cecil B. Jacobson, MD	told women they were pregnant, used his own semen for DI, moves for a new trial, appeals the case
anonymous donors	(no explicit activities)
judge	sentencing, fining, commenting
women patients	suffer emotional anguish and psychological trauma

Thus, the summary gives the reader the framework of a particular type of story which we 'naturally' want to hear more of - a court case involving criminal, victim, and motive.

The paragraph is obviously densely populated with people and their activities appropriate to such a story. This in itself does not guarantee the need for further comment, or lend itself to establishing a moral. However, I think the paragraph does accomplish this and we can

certainly identify one important sentence involved in this work. This is the sentence which presents the reported speech: "the judge commented how he 'had not seen a case where there had been this degree of emotional anguish and psychological trauma' " (ll. 12-14). Note how the sentence builds up the expectation of a forthcoming moral, but further, it tells us something about the sort of moral we are looking for. My feeling is that it says, 'this is a big case, look for a big moral'. This is reinforced by the previous presentation of the information that the target audience is **everyone** practising assisted human reproduction, not, for example, everyone practising donor insemination, or, people practising assisted human reproduction in the United States.

Further, the statement is accredited to someone with believable authority to make such statements - the judge. The quoting of the judge's statement also does some important naturalised recategorisation work. When we read the statement we do not understand from it that it is Jacobson who suffers the anguish and trauma, nor the lawyers, nor the jury, nor the court stenographer. We know, precisely, that it is the women patients who were told they were pregnant, and who were inseminated with Jacobson's semen, that suffered the anguish and trauma. Hence, they have moved from being "women patients" to victims. The correlate of this recategorisation is that the reader is encouraged to read on looking for more details of the story. It is a story with a compelling readability in our culture -the narrative of criminal and motives, victims, and the pursuit of justice within the courts.

The next five paragraphs elaborate upon the points presented in the first summary paragraph. Respectively, they are concerned with: legal particulars; Jacobson and a brief history of how his crime was discovered; the legal specifics of the charges; who testified at the trial; motive; motive and the verdict. All this constitutes a large amount of specific detail on the mentionable, and given that the story can be situated within a common narrative framework it is 'natural' that these details be provided.¹⁴ But, a possible problem with the provision of so much detail is that the moral is lost to sight. However, I would assert that the reader does not get lost along the way and forget that he or she is reading with a mind to the forthcoming moral. We are not surprised when the moral does appear in the final paragraph, and it does not seem out of place with all that has gone before. The transition is successfully accomplished. Just how successful the editorial is in solving the transition problem can be seen if we count the number of lines devoted to the mentionable - 84 (ll. 2-86) - and compare it to the text devoted to the moral and target population - 23 (ll. 1-2, 87-107). That is, the text devotes nearly eighty per cent of its space to the mentionable, and yet the moral is clearly drawn in the remaining space and the link between the two parts does not seem strained or tenuous.

A very efficient device for accomplishing this continuity occurs at the beginning of the moral paragraph, and I will discuss this shortly, but before this there are various keys within paragraphs two to seven that ready us for the transition from mentionable to moral. The first two sentences of paragraph three (ll. 24-26) seem particularly

¹⁴ Also, given that *Human Reproduction* is published primarily for a European audience, there is some need of details of the American court system. It could be inferred that this is related to the enlistment of a law professor as co-author.

important. The first tells us that "In the early 1970s Jacobson built a worldwide reputation as an innovator in human infertility". Prior to this we have encountered our protagonist as Cecil B. Jacobson, Cecil B. Jacobson MD, he, his, and Dr Jacobson. Up to this point then, we have minimal information on Jacobson himself, and therefore it seems relevant to ask why this information is presented in paragraph three and not earlier.¹⁵

I believe the reason for the late presentation of this information lies in the tacit guidance work the text contains. The reader is being encouraged to read for a moral which has to do with the organisational nature of medical activities, and not to read for an editorial on medical practitioners themselves. Hence we read twenty three lines with minimal information on the mentionable's protagonist, and then we are presented with the considerable knowledge that he was an "innovator in human infertility". We know from this that we are not dealing with a medical conman; but crucially, this information is immediately juxtaposed in the next sentence with the further details that "His work was not done in a hospital setting, so institutional oversight was absent" (ll. 25-26). These sentences are an unusual pairing: when I read a first statement that someone is an innovator or a leading researcher in some field, I then expect to read details of just what they did and just how it constituted innovative or leading research. Instead, the next sentence moves from Jacobson's qualities as a researcher to the organisational nature of his research activities. The notable term "institutional oversight was absent" seems to retrospectively alter the sense of "innovator" given in the first

¹⁵ For example, the editorial could have started 'Cecil B. Jacobson, a recognised innovator in the field of human infertility research, was recently convicted' and so on.

sentence. It overshadows the information that Jacobson is an innovator in human infertility research, and seems to focus the reader towards a moral on medical institutions and accountability, rather than on Jacobson himself, or the common pressures that doctors face.

The opening sentence of each paragraph in the mentionable section further reinforces this focusing work. The pattern is to focus on the legal proceedings as a whole rather than the individual actors within. This is done in a very orderly manner, as the beginning of each paragraph opening sentence shows:

- "The trial of a case ..." (l. 1)
- "Conducting the case ..." (l. 16)
- "The local United States Attorney ..." (l. 43)
- "At the trial ..." (l. 61)
- "Motive and motivation kept appearing in the case." (l. 75)
- "Under the judge's instructions, the jury..." (l. 87)

The only exception to the pattern of focusing on the trial in the paragraph opening sentence is paragraph three. As I noted above, this does begin with Jacobson but then retrospectively focuses on institutional accountability. So, in addition to the initial ordering function of the caption and title, the text displays mini sequential ordering devices throughout. Paragraph openers could be seen as a nested instruction within the overall instruction provided by the caption and title, that is, they provide indications of how the contents of individual paragraphs should be read: 'keep looking for details of the "case" or "trial" or the "judge's instructions"'. Collectively, this is an effective device for solving the transition problem, which is specifically concerned with moving from an individual doctor's crime to a moral about institutional accountability.

The only other place where the text opens up on Jacobson himself is on the issue of motive. It is worth considering how the text deals with this potential problem before we finally consider the transition to the moral paragraph. I say it is a potential problem because some discussion of motive appears to be 'naturally' required in any story with the narrative contents of criminal, victim, and court proceedings. It could also be reasonably suggested that the nature of the 'crime' seems to add to the natural quest for motive: the question of why Jacobson substituted his sperm for that of anonymous donors is an interesting one. Overall, readers could quite naturally be looking for some discussion of motive, and it does come, in orderly fashion after the details of the criminal, victims and court proceedings, in paragraphs six and seven. My impressionistic feeling is that these paragraphs are crucial to the successful accomplishment of the editorial, not so much for what they contain but for how they avoid being drawn into an extended discussion of motive in its own right.

Motive is dispensed with quickly, essentially by following the pattern of focusing on the legal proceedings rather than the individual actors. Paragraph six opens with the statement, "Motive and motivation kept appearing in the case" (l. 75). Thus, although the opener signals to the reader that we are finally to be offered some discussion of motive, we are also given a framework for the discussion - it kept appearing in the case. Consistent with this, paragraphs six and seven restrict themselves to details from within the case. We are presented with summaries of motive-talk from: "some of Dr Jacobson's patients" (ll. 75-76); "The jury foreman" (l. 81); and "An ex patient" (l. 84). In a very neat manner the text avoids

presenting evaluations of the 'truth' of the various motive attributions, instead they are presented for their belongingness to the case - they are what happened in the case. This seems to be a crucial point in the editorial: to have changed footing and entered into an authorial discussion of motive would have been an extended matter, and surely one which given current dominant understandings of motive would have involved a heavy emphasis on individual psychology. There is a trace of this in the paragraph: Jacobson is described as a "distinguished medical researcher, married for 30 years to a loyal wife, a religious man with seven children in the family" (ll. 78-80), but any further consideration of how his character traits might be related to motive is quickly suspended and the text immediately returns to what happened in the case. Thus, the potential problem of motive is dispensed with, and the editorial keeps on track to the moral and its emphasis on organisational accountability.

An economical transition to the moral is made via the very effective device of beginning the final paragraph with a question: "Could similar events happen in clinics elsewhere?" (l. 96). The term "similar events" takes the whole prior explication of the mentionable and treats it as a signifier - we do find after all that we were not interested in the events themselves, but in their function of pointing to the possibility of "similar events" occurring. And the phrase "clinics elsewhere" does not pose a question like 'clinics just where?', for we know from the first sentence that the target population is "everyone practising assisted human reproduction", hence, "clinics elsewhere" refers very efficiently to 'clinics everywhere'. So, in one short move this sentence takes the reader from the previous details of the

mentionable to a concern for the upcoming moral, and in the process reinvokes the target population.

In terms of form, the opening sentence of the final paragraph is a question - one of only two that the text uses. Of course, as a question the sentence relevances the production of an answer. However, as this is written discourse there is no possibility of a direct interactive answer coming from the reader, so the question seems akin to a "formulation", that is, a point in discourse where what is reflected upon is the discourse itself - what the question is doing is "saying-in-so-many-words-what-we-are-doing" (Garfinkel & Sacks, 1970: 351). Although it is not quite a formulation - 'we are now going to make the moral' - it does very effectively signal that the moral is about to be made.

Further, as a question it very effectively draws the target population, or any reader for that matter, into the making of the moral. Note that while the question has obviously been posed by the editors, there is no break from the "transcendental narrator" style (Morrison, 1981), that is, the answer is not 'We do not think so' - no pronoun is introduced. The first part of the answer - "It is easy to assume they could not" (ll. 96-97) - keeps the answer at a general level and incorporates the target population in the work of assuming. It is not the editors that are seen to assume, but "everyone practising assisted reproduction", or even 'Anyperson reading the editorial' that assumes. This collaborative assuming work means that it is an easy matter to make the moral and proceed to a closing.

Indeed, the closing is done most economically in eleven lines. Within these eleven lines there is another 'It' statement - "it is to be hoped that this practice spreads wider" (l. 103) - that again draws the reader into the making of the moral. This accomplishment of collaboration seems crucial to the overall success of the text as an editorial. Editorials face a problem of limited textual space, and additionally may face problems of warrant: given that in the above editorial a general point is being made from one specific case, there is the issue of the validity of such generalisation. Hence, it is important to get the reader collaborating in the making of the moral.

That the editorial succeeds in this task is indicated by the fact that while it calls for accountability to ensure that assisted human reproduction is "conducted properly", no discussion at all is offered as to what proper conduct might consist of, nor to who will "establish the standards". But of course, it does not need to: the editorial in its wholeness provides tacit knowledge of just what such proper conduct is, and at the very least we know that it is not the type of conduct that Cecil B. Jacobson indulged in. In a logical sense the editorial is relatively vague, but the point is it is not read in a logical detached manner. By successfully utilising various taken-for-granted orderly properties, the editorial very clearly describes a mentionable and makes a moral which 'Anyone' can readily understand.

In sum, the editorial is successful in accomplishing editorialising. It makes very clear its moral and to whom it is applicable, all within the space of 107 lines of text. While the above analysis has been relatively simple, the strength of the general principle underlying it should not be underestimated. In essence, it is this. If in a text like

that above, there is an indication of an overall task the text is about to accomplish, then any particular part of that text will be interpretable in relation to the overall task. In the editorial, the relationship between the mentionable and the moral pervades the whole text in a back and forth reflexive manner: particular words, sentences, and paragraphs are read in the light of the underlying concern to make a transition to a moral, and the solution to the transition problem is accomplished using the aspects of the mentionable as manifest in particular words, sentences, and paragraphs.¹⁶

3.5 *Conclusion*

As noted above, the two sections in this chapter are to a large degree separate. The analysis of the editorial is not intended to show that the male reproductive system is just as amenable to "negative" description as the female reproductive system, nor any other rebuttal of Pfeffer, Beagan, and Steinberg's arguments. But, at a general level, the analysis can be contrasted with the feminist arguments, and a brief reiteration of this point is a useful way to finish this relatively long chapter.

To recap, I outlined how Pfeffer, Beagan and Steinberg argued that the language of reproduction was doctored to further the subordination of women and the superordination of men in society. In specific they argued that: the male reproductive system is treated with "linguistic kid gloves"; semen is described positively;

¹⁶ Of course, this is merely a reiteration of the "documentary method of interpretation" (see Garfinkel, 1967).

practitioners and procedures are emphasised at the expense of women who are depersonalised; the language of the new reproductive technologies captures the infertile and focuses on women; there is no authentic language to describe women's experience of reproductive technologies. For data they used various extracts from medical discourse: textbooks, phrases, abstracts, scientific terminology, and so on. My critique covered the variety of arguments made, but essentially I argued that the language of reproduction was only 'doctored' to the extent that it is produced by and for doctors, and it assumes and enacts the background knowledge of that existential world.

Following from this critique, in the analytic section I avoided a substantive rebuttal of Pfeffer, Beagan, and Steinberg's arguments. Instead, I took an editorial from a medical journal that I chanced upon and showed how it accomplished its intended aim -editorialising - by using held-in-common social resources. That is, the focus was on how the editorial was "done together", how it was a social activity. The social resources I focused on were primarily those of sequencing, and the overall interpretative procedure of relating underlying pattern and particulars (the documentary method). Via such an approach, order was found in the text itself. The argument is that the editorial's competence as an editorial is locally produced; its objectivity as an editorial which makes-a-moral-from-a-mentionable is accomplished by readers using social resources applied to what they have in front of them in the text. This stands in contrast to approaches which bring external structural forces to texts and argue that these forces explain discursive features of texts. While it could be argued that the editorial does other things, for example, hiding accountability for

medical misdemeanours within the realm of medicine itself, first and foremost the text must accomplish editorialising, and do so locally.

Essentially, it is the importance of local production that Pfeffer, Beagan and Steinberg have neglected. Every piece of data that they present is an extract, firstly, from some whole text - paper, chapter, book - and, secondly, it is extracted from some local organisation - a particular journal and its readership, a particular medical specialty and its practitioners, a particular medical body, and so on. The point pure and simple is that the extracts Pfeffer, Beagan and Steinberg use, receive their sense, their structure, their order, from these first two levels and **not** primarily from any aspect of social structure - patriarchy, medicalisation, depersonalisation, and so on - that they bring to the texts from their stock of second order concepts.

As moral citizens we are certainly free to debate the 'politics of reproduction', but as analysts using socially organised discourse, whether it is written by scientists, doctors, nurses, technicians, or infertile people, we have an obligation to treat it as an embodied production of an existential world. That is, we must avoid treating it ironically.

Of course, this is a standard ethnomethodological argument, one that is traditionally framed in the distinction between "topic and resource" or between "constructive analysis" and ethnomethodological studies. While there are good grounds for arguing that ethnomethodology is not immune from the topic-resource problem (see McHoul, 1982: ch. 4), at a technical-methodological level the point still seems cogent. However, it must be noted that

cogency at this level incurs a price. The pressing issues of the 'politics of reproduction' must be held in check until attention is given to documenting the socially organised nature of the practical activities that make possible both 'politics' and 'reproduction'. That price may seem too high for some, but I would endorse Crook's comment (on another issue) that, "however heavy that price may seem to be, it must be paid" (1989: 376).

Chapter Four

From 'the Serious and the Humorous' to 'Doing Humour, Seriously'

4.1 *Introduction*

Academic discourse on infertility very often begins with statements like, 'Infertility is a major life crisis'. Consider the following statements culled from the opening passages of academic papers: infertility, "involves profound biographical disruption" (Sandelowski, Holditch-Davis & Harris, 1990: 195), "ranges from a minor irritation to a major life crisis" (Link & Darling, 1986: 46), is "a major biosocial life crisis that also represents a serious threat to the development of psychosocial generativity" (Snarey *et al.* 1987: 593), has "profound psychosocial impact" (Greil, Leitko, & Porter, 1988: 172), "can provoke a complex *biopsychosocial* crisis that may take several years to resolve" (Piel Cook, 1987: 465, original emphasis), and, returning to more mundane language, is "a common life crisis" (Abbey, Andrews & Halman, 1992: 408). It is not difficult to think of a sociological basis for this view of infertility as a "life crisis". Without doubt, children have a very positive value in modern society, indeed, it has been argued that children have become "emotionally priceless" and "sacralised" (Zelizer, 1985). If this is even partly true, infertility could be a severe disruption to people's lives.

But, alongside the seriousness of infertility, there is a lighter humorous side. This side became obvious to me in the process of interviewing infertile men and health professionals, observing infertility society meetings, and collecting textual material on infertility. Jokes, laughter, puns, wisecracks, and funny incidents, were remarkably pervasive features of the experience of infertility. No matter how serious the matter being dealt with, people are able to find room for humour.

Further, such incidents of humour need not be mere asides or afterthoughts. They can have a good deal of time devoted to them and might occur right in the midst of serious aspects of infertility. A good example occurred at the 1991 annual conference of the New Zealand Infertility Society. The conference was formally opened with a welcoming address, and within seconds of beginning, the speaker - a middle aged woman - began quivering and broke into tears. She explained her tears by saying that infertility was obviously an emotional matter. The audience displayed a hushed attentiveness throughout her address. In contrast, the end of the two day conference involved a debate on the topic 'Infertility: is it in the heart or the gonads', where, if anything, the participants were brought to tears by laughter. The debate featured skilled debaters who took every opportunity to make jokes and puns, almost universally on a sexual theme. This was complemented by the presence of a large anatomical model of the human heart, featuring amazingly breast-like and penis-like parts, which, of course, were used as 'evidence' in the debate.

There were other humorous incidents throughout the conference, but the debate was by far the most sustained period of humour. This identifying feature of the debate might therefore invite sociological questioning. One possible avenue is a functionalist argument that the debate functioned to ease the seriousness of the preceding parts of the conference, hence contributing to its overall success. This form of explanation could be extended to the point noted above, that is, people frequently find room for humour in their experience of infertility: given the seriousness of infertility in our society, humour functions to make the difficulty of infertility more bearable. While these statements are simplistic, more developed forms of such argument have been the staple of functionalist explanation for many years, and there is little doubt that such arguments have a certain explanatory power, for both lay people and academics.

However, in the last two decades at least, functionalist explanation has fallen out of favour as a result of sustained criticism (see Giddens, 1984). Given this context it is therefore very surprising to find an essentially functionalist argument prevalent in one of the few book-length sociological studies of humour - Michael Mulkay's *On Humour* (1988). While Mulkay's book is an eclectic attempt to synthesise various theoretical perspectives - including ethnomethodology - in the analysis of humour, overall, it makes a functionalist argument. Below, in the first part of this chapter, the functionalist aspect of Mulkay's study will be detailed and critiqued. In the second part of the chapter, the explication and critique of

Mulkay is used as a basis for an original consideration of humour within discourse on male infertility.

4.2 *Mulkay on Humour*

Rather than offer a literature review of sociological work on humour, here I will focus solely on Mulkay's study. The main reason for singling out this work is that Mulkay's argument is centrally concerned with an area that ethnomethodologists have paid particular attention to - mundaneity, the ordinary world, everyday life. Indeed, Mulkay utilises ethnomethodological studies in a frequent and detailed way. For specific research on aspects of humour he uses Sacks (1978), Jefferson (1979, 1985), and Drew (1987); for its more general applicability he uses Pollner's work (1974, 1975) on mundane reason; and he also cites Garfinkel (1967) and Heritage (1984). Consequently, the cogency or otherwise of Mulkay's argument is of interest given this study's framework. In addition, it should be noted that Mulkay's study is the most recent, in fact one of the few, book-length sociological studies of humour. Further, Mulkay's credentials as a sociologist are good: he is a leading researcher in the sociology of science (see 1991) and has produced some interesting thoughts on the form of sociological investigation (1985).

Despite the distinctive form of this latter work, Mulkay opts for a very traditional approach in his study of humour. He begins routinely by commenting on the lack of research on his chosen topic: the apparently universal phenomenon of humour has been neglected by

sociologists. As a possible reason for this neglect he suggests that sociologists may have confused the 'non-serious' with the 'trivial'. With this lead-in he then sets out the central theme of his study:

They [sociologists] may have assumed that humour, because it is by definition outside the domain of the serious, is not worthy of serious investigation. The central theme of my analysis, in contrast, is that the exact opposite is the case. I will show that it is precisely the symbolic separation of humour from the realm of serious action that enables social actors to use humour for serious purposes, and that makes humour an essential area for sociological inquiry. Humour is of interest, not only in its own right, but also because its study helps us better to understand our serious social world. (1988: 1)¹

From the first page then, Mulkay sets out a bi-polar model consisting of what he calls "the humorous mode" and "the serious mode". This introductory set-up provides Mulkay with two clear tasks: firstly, detail how the two realms are different, that is, make a good case that they really are different; but secondly, show how the two are related. In a twelve chapter book these core tasks occupy chapters one, two, and twelve; the remainder are mainly concerned, to use the subtitle of the book, with humour's "place in modern society". The following discussion will not touch upon Mulkay's analysis of humour's place in modern society, but will focus on his core theme. After describing this, some fundamental conceptual problems with Mulkay's argument will be identified.

¹ From this point on numerals in brackets without a year date will reference Mulkay, 1988.

Distinguishing the Humorous from the Serious via "Plausibility Requirements"

In his attempt to characterise the humorous mode, Mulkay begins by noting that there are inherent difficulties in analysing humour seriously. A major risk is that in treating what is by definition non-serious as serious, humour is distorted beyond recognition. However, this problem should not be seen as insurmountable, and to illustrate this Mulkay discusses Sacks' analysis (1978) of a dirty joke's telling. He suggests that Sacks' use of naturally occurring data is a good first step in alleviating the risk of distorting humour, and, in general, he commends Sacks' analysis as an example of how humour can be analysed sociologically and shown to be a finely organised social activity.

But a further reason that Mulkay explicates Sacks' analysis in such detail - at least nine pages - is that he wishes to take issue with the way Sacks comments on the relationship between jokes and implausibilities. Rather than try to explicate what Sacks 'really' said, or what Mulkay 'really' said about Sacks' analysis, I intend here to focus on Mulkay's introduction of "plausibility requirements" and for the moment simply gloss what Sacks said. The gloss is: Sacks commented that the structure of dirty jokes hid away implausibilities in the chain of events utilised to produce humour.

Sacks' argument will be returned to in the concluding discussion, for now, it is sufficient to note that despite a generally positive evaluation, Mulkay accuses Sacks of falling into the trap of

misframing humour by approaching it with serious analytical devices. Instead, what Mulkay wishes to do is

... build upon this part of Sacks's analysis by proposing that, as participants, we approach humour with an interpretative procedure which differs significantly from that applied to serious discourse. When we listen to a joke, we do not expect or require that the events depicted should exhibit the same kind of interpretative consistency as that to be found in serious conversation. I suggest that we distinguish humorous from serious discourse and that we employ what we may call different 'plausibility requirements' within these two discursive modes. (17)

Basically, Mulkay is suggesting that the "plausibility requirement" of the humorous mode is "anything is possible" (19). Because of the adoption of an interpretative frame suitable to the humorous mode, we do not expect that the events depicted will exhibit the interpretative consistency found in the serious mode.

To reinforce this argument Mulkay provides the example of two jokes,² where in contrast to Sacks' assertion that implausibilities are hidden away, the implausibilities are inescapable. It is worth reproducing the jokes here:

1. What do you get if you cross an elephant with a fish?
Swimming trunks.
2. 'The dapper man'
On the first evening after moving house, Bob went down to the local pub and started talking to the barman. After a while, their talk was interrupted by the arrival of a dapper little man, evidently a regular, who greeted the barman,

² Note that despite identifying Sacks' use of naturally occurring data as one way that analysts can avoid the problem of the serious analysis of the non-serious, Mulkay does not heed his own advice. That is, he chooses to compare two 'written' jokes with Sacks' written representation of a joke's telling. While the 'written' jokes certainly contain the material for the telling of a joke, or vice versa, actual verbal performance of jokes will always be different from written jokes.

ordered a glass of sherry, drank it, said goodnight, walked up the wall, across the ceiling, down the opposite wall, and out through the door. There was a short silence before Bob said, quaveringly:

'Wow! *That* was strange'.

'Yes', mused the barman. 'That *was* strange.

He usually drinks whisky'. (Nash, 1985, p. 107)

(17-18)

With reference to these examples, Mulkay observes that humour often depends upon incongruity or implausibilities. We must assent to the occurrence of utterly implausible events for jokes to come off - in the examples, crossing a fish and an elephant, and a man walking across a ceiling. This is taken as an illustration that jokes operate outside the assumptions and expectations of serious discourse. Humour, supposedly, creates its own "framework of expectation":

Participants are surely aware that the events depicted in a joke occur within the special world of humour where, in principle, as long as the speaker sustains the humorous mode, *almost anything can happen*. ... These impossibilities are possible because humour is marked off from serious discourse; and because the requirements of acceptable discourse vary from one mode to the other. ... humour operates according to plausibility requirements that are quite different from, and much less stringent than, those operative in serious discourse. (19-20)

Thus, Mulkay is arguing that the humorous mode has objectives and principles that are fundamentally different from, and alien to, the serious mode. Content that he has made this clear, and in the process improved upon Sacks' analysis, Mulkay moves on to detailing the basic characteristics of serious discourse.

In this progression Mulkay offers a very brief discussion of some of Pollner's work on mundane reason (1974, 1975), and then a

reiteration of the difference between the humorous and serious modes. Mulkay uses Pollner's work to fix the core characteristic of the serious mode as the assumption of a unitary world: people assume that they inhabit a real social world, and assume other people share that view. This is not to say that there are never disagreements, contradictions, or ambiguities. These do occur, but significantly, they are treated as problematic and they will be resolved by reference to the assumption that there is still an underlying, unitary, real world.

Mulkay makes two main uses of this characterisation of serious discourse. The initial point is that it is precisely the assumption of a unitary world that the humorous mode reverses:

In the realm of humour, recipients are not expecting or seeking congruity. For they have temporarily abandoned the assumptions of the ordinary world and are responding to, registering and celebrating a world of discourse where interpretative duality is the basic principle and understandable incongruity the overriding aim. (37)

The second point, which in the final chapter becomes the apogee of Mulkay's central theme, is that it is supposedly in the core characteristic of the serious mode that we see the ultimate reason for the existence of humour. The argument proceeds as follows: people both, differ from each other, and, have different experiences within their own everyday life; and even though they all assume the existence of a unitary real world, these differences result in the existence of **multiple formulations** of the real world:

In other words, the basic structures of social differentiation that occur in all societies generate a potential babble of discrepant voices, each of which speaks as if its particular version of the

world is the real world within which all other voices have their being (214)

Thus, Mulkay sets up modern life as fundamentally paradoxical and inherently unstable due to individual differences and the tendency to 'see things my own way'. All this leads to Mulkay's core explanation: "Humour occurs because mundane, serious discourse simply cannot cope with its own interpretative multiplicity" (214).

That is, we seem to be trapped within an 'iron cage' of mundane reason, and within this cage it is humour that allows us to go on:

When we adopt the humorous mode, we are able to give voice to the deficiencies of serious social life and of our serious procedures of language-use, whilst at the same time disregarding them. By signalling that we are abandoning the serious mode, we indicate that there is a legitimate region of the social world in which the requirements of that mode are inapplicable. Having signalled our departure, we are not obliged to take seriously whatever follows; that is, we no longer have to employ a unitary, internally coherent speech. ... Humour furnishes a realm of safety and release from these problems. ... [In humour] the onerous duty of maintaining a unitary world-view has been replaced by the joyous creation of multiple realities. (214-215)

Essentially, Mulkay's argument is an 'escape attempts'³ thesis: humour allows escape from omnipresent, oppressive, and always serious, reality.

4.3 *Critique*

Mulkay's analysis of humour is certainly interesting and broad ranging, nevertheless, there are several dissatisfying aspects to his

³ This term is taken from Cohen and Taylor's *Escape Attempts: the theory and practice of resistance to everyday life* (1976).

argument. It is not easy to set these out briefly, but I will attempt to do so by making two main points in critique. First, problems will be identified with the notion of differing plausibility requirements and, second, with the 'multiple formulation of reality' argument and its utilisation in a functionalist explanation of humour. After these critical points the chapter moves on to an analysis of humour in discourse on male infertility.

To begin, I want to adapt a conceptualisation from Sharrock and Turner (1978). In their paper based on a corpus of telephone calls to a metropolitan police force, they identified a subset of "complaint giving" calls. The useful conceptualisation they introduced was to refer to the phone calls as conversational events as a "complaint" and to use the "less euphonious term *complainable* to refer to the state of affairs a complaint formulates" (1978: 174). Adapting their logic, the term 'joke' is used to refer to the conversational telling of a joke, or jokes in textual discourse, and the (again less euphonious) term 'jokeable' is used to refer to the events a joke formulates.⁴

Problems with Plausibility Requirements

There is little doubt that as members we know that a joke is different from serious discourse. There is certainly some ground here for Mulkay's argument about differing "plausibility requirements".

⁴ I have introduced the terms joke and jokeable rather than humour and humourable because my critique is essentially focused on the argument that begins in Mulkay's first chapter, and here he focuses on jokes. Besides which, humourable is a ghastly phrase.

However, if unpackaged a little, it can be shown that his argument suffers from a problem of infinite regress.⁵

Mulkay's model of humour as a discursive event is quite rightfully predicated upon a view of the human actor as an interpretive being. However, we are presented with a relatively static model of interpretation which utilises a type of 'macro-micro' framework: at a 'macro' level Mulkay talks in a very Goffmanian way of 'frame' shifts to and from the serious and humorous modes (and note that there is very little attention to how these shifts are actually achieved and maintained). At a 'micro' level these frame shifts supposedly set up very clear expectations of what will then be done within the new 'frame'. This is all presented in very static terms as, essentially, information processing: as cognitive beings we use appropriate "interpretive procedures" to select the frame-appropriate "plausibility requirement", within which we process information (jokeable) to reach the pre-set outcome of joke-acknowledgement.

When he adds into this model the quite reasonable assertion that humour seems to depend upon incongruities and implausibilities we see a logical problem in Mulkay's argument. The problem begins when we realise that no matter how short humorous discourse is, it must contain some talk of plausible events - you cannot get to implausible events without first moving through some unproblematic and ordinary events. To take Mulkay's example of the joke about

⁵ This argument is inspired by McHoul's critique (1983) of information systems models of human communication, a model to which Mulkay's analysis of humour is remarkably similar.

crossing elephants and fishes, the real world action of "crossing" is not to be treated as implausible - it is unproblematically available as a serious practice that actually happens.⁶ So, when Mulkay suggests that the plausibility requirement of the humorous mode is "anything is possible" he omits at least three essential words which render the statement meaningful: 'anything is possible **amidst the normal**'. He does hint at this realisation when he suggests that "understandable incongruity" is the "overriding aim" of humour (37), but he does not pursue this further.

Mulkay's model is of a cognitive actor reading information in a joke, recognising that it is implausible, that is, checking in a correspondence fashion with knowledge of the real world, but letting the implausibility pass because he or she is in humorous mode. But if he wishes to argue in this manner he must also be implying the recognition of plausibilities. This is a logical consequence of his emphasis on interpretive procedures (and also his loose adoption of a model of cognition as information processing): one cannot have the recognition of implausibilities (not A) without the recognition of plausibilities (A) - they are a duality and not a dualism.

If one accepts the above point then we can see how such a presupposition leads to an infinite regress. To reiterate, Mulkay has set up a model where a mode of discourse produces a frame where the interpretative process is to accept implausibilities and indeed to expect them. I have added the fact that this model of the interpretive

⁶ Obviously if the joke is told to someone who does not know what "crossing" is, it will not 'work'.

process includes recognition of what is normal, or in other words, plausible. Thus, this view holds that the interpretive process(es) involved in the humorous mode involve a monitoring for both plausibility and implausibility. The trouble is, to paraphrase Ryle⁷, the consideration of plausibility/implausibility is itself an operation which can be more or less plausible, less or more implausible. But if, for any operation to be plausibly executed, a prior theoretical operation had first to be performed plausibly, it would be a logical impossibility for anyone to break into the circle.⁸ As McHoul puts it (read hearer for speaker),

If a speaker is assumed to "look up" those criteria then the same criteria must apply recursively to the "look up" rules. And so rules for the application of rules could be specified, recursion upon recursion. This must be a consequence of any theory which separates thought and language - a self-annulling consequence of the theory. (1983: 284)

I believe that this is exactly what Mulkay has done: separated thought (interpretive processes/plausibility requirements) from language (humorous discourse). Hence, ultimately, Mulkay's theoretical explication of humour will have an unknown relationship to the actual interactional accomplishment of humour. The way Mulkay frames his argument is heavily dependent upon cognitive processes which are exogenous, or additional, to the interactional accomplishment of humour. Just how can an analyst satisfactorily identify "plausibility requirements" within empirical materials? In the

⁷ As quoted in McHoul (1983: 284).

⁸ See Coulter (1991) for a more general formulation of the ethnomethodological critique and respecification of cognition. He presents a similar 'infinite regress' argument about views of language as mental/neural processing.

absence of a good answer, it seems more worthwhile to search for modes of analysis not so heavily reliant on exogenous factors.

Multiple Realities and the Function of Humour

It is clear that the background to Mulkay's discussion of the characteristics of serious discourse is the work of Alfred Schutz. Even though Mulkay does not discuss Schutz's work directly, the path from Schutz to Pollner to Mulkay is clear enough, and is acknowledged (see 22-24). However, in the transition something seems to have been lost: Sharrock and Anderson's (1991) succinct discussion of Schutz leads me to the conclusion that Mulkay has made some fundamental mistakes in his treatment of serious discourse, multiple realities and the function of humour.⁹

In the beginning of his discussion of the characteristics of serious discourse, Mulkay keeps with a key import of Schutz's argument, that is, that doubt, ambiguity, and contradiction occur within the natural attitude but do not jeopardise the assumption of an external real world. To recap, Mulkay argues that despite commonsense providing us with a notion of one external world, each individual formulates it in their own way, and each believes their formulation to be correct. From this it is argued that humour functions as a release from the tensions created from living in a paradoxical state - humour allows the creation and celebration of multiple realities. Thus, there are two

⁹ I am not saying here that Mulkay has misread Schutz, for he never claims to base his arguments on Schutz, only Pollner, and hence Schutz secondarily. What I am saying is that a consideration of Schutz, like that provided by Sharrock and Anderson, casts into doubt the way Mulkay has formulated serious discourse and used this in his argument about the functions of humour.

areas where Schutz seems particularly relevant to Mulkay's argument: ego and intersubjectivity; and talk of multiple realities.

As Sharrock and Anderson forcefully argue, Schutz did indeed start his theorising from a concern with intersubjectivity (and hence subjectivity itself), but he was a long way from arguing that the natural attitude is ego-centred, or that individuals assume the external world revolves around them. On this point, they present a cogent analogy: people speak of 'my home town' in the sense that it is the town in which they grew up or currently live, hence, it is not that the town belongs to the speaker, but the speaker belongs to the town - the town may be the centre of the speaker's life, but the speaker is not the centre of the town's life. Talk of 'my world' is made in the same sense as talk of 'my town' - to say that the social world is, in the natural attitude, encountered as 'my world', is not a statement of ego-centredness. Even though we each have a unique experience and may express this through utterances such as 'my view', this in no sense implies that each individual holds his or her view to be the correct view of reality. The cogency of this position is seen very easily by considering our everyday lives: it is very rare to experience life as a "babble of discrepant voices", as Mulkay would have it.¹⁰

Schutz theorised that a large degree of the orderliness of social life was derived from intersubjectivity. He made two crucial theses: 'the

¹⁰ A very powerful analysis which illustrates this point is provided by Schegloff (1992). He shows how our culture provides very strong devices which members can call on to maintain intersubjectivity. His specific analysis is of "repair after next turn", which he calls the "last structurally provided defense of intersubjectivity in conversation". This could be thought of as a common mechanism for producing mutual understanding and avoiding Mulkay's "babble of discrepant voices".

reciprocity of perspectives' and 'the interchangeability of standpoints' (Sharrock & Anderson, 1991: 66). In essence, these theses hold that, while person A will not see things in exactly the same way as person B, they will reciprocate each other, such that differences will be attributed to different locations. 'Interchangeability of standpoints' refers to an assumption that if A and B were to change places, what A saw would now be what B sees, and vice versa. These assumptions are held to operate in the natural stance, but the crucial point is that they are assumptions - they are not guaranteed and it is known that they do break down. Thus, it should be emphasised that Schutz formulated these two theses from an ideal starting point - they have legitimate application only when it is assumed that biographical differences are non-existent. The point is that individuals do know that others differ and have different life experiences and this knowledge does not lead, as Mulkay would have it, to the formulation of multiple realities. Rather, it is incorporated into the natural stance: "allowance for the diversity of experience and culture is built into the socially distributed stock of knowledge itself ... (Sharrock & Anderson, 1991: 66). Built-in knowledge of diversity does not alter the utility of the 'reciprocity of perspectives' and the 'interchangeability of standpoints', but rather strengthens them as the baseline of intersubjectivity.

If Schutz's argument is accepted, then there is no warrant for setting up everyday life as a paradox where individuals operate with a reciprocal assumption of one real world, but which they selectively apply to their own formulation of reality. Nor, as Crook (1989) notes, does 'indexicality' present a similar problem of multiple viewpoints -

people swimming "in a thick soup of immediacy [where] salient 'meanings' remain implicit, and incapable of explicit formulation" (1989: 377, fn. 8). As he notes, quite frequently in our encounters with formal organisations, for example, we are met with explicit requests that meanings, opinions and information be formulated explicitly for the practical purposes of the organisation at hand. When, within such organisations, disjunctures, ambiguities, puzzles, and contradictions occur, remedial action will not be to demand the correctness of an individual's viewpoint, but to demand that **anyone and everyone** sees things in a particular way given the local organisation's knowledge. As Sharrock and Anderson note, Schutz's work emphasised the "experiential underpinning of the *socially sanctioned* unity of the world, of the mutual *demand* that we recognise the commonality of circumstance" (1991: 65, original emphasis).

This point about the "socially sanctioned unity of the world" was an important one that ethnomethodology subsequently incorporated in its emphasis on social facts being both natural and moral (see Heritage, 1984, ch. 7). This seems to be a point that Mulkay has picked up from Pollner, but misconstruing it, has turned it into an 'escape attempts' thesis. That is, a view of individuals tiring of adhering ceaselessly to the natural-moral accountability of the real world and resorting to humour as a major device which allows them to proliferate opposing (and "joyous") multiple realities. But, by returning a last time to Schutz and what he said about multiple reality and paramount reality, some fundamental problems with Mulkay's position are apparent.

Schutz clearly takes a constitutive view of reality: "... we emphasize that it is the meaning of our experiences, and not the ontological structure of the objects, which constitutes reality" (Schutz, 1962: 341). With this as a base, he argues that meanings can be subdivided into "finite provinces" and it is in this sense that he talks of "multiple realities". However, he also identified the world of everyday life in the natural attitude as "paramount reality". But again there is nothing ontological about this conceptualisation: the point of differentiating finite provinces of meaning is to emphasise the episodic nature of experience. In any one day we typically move from periods of sleep to periods of wakefulness, between moments of unreflective activities and moments of thoughtful reflection, between leisure and work, and so on. Sharrock and Anderson (1991: 64) argue that there are four important points that Schutz was making here:

1. the transition between these episodes is abrupt, with each sphere being self-contained;
2. **within** each sphere an 'accent of reality' is assigned to the things experienced there;
3. the transition from one episode to another is typically via 'paramount reality';
4. the standards of reality which are applied in paramount reality pre-empt those in the other provinces of meaning.

Keeping with Mulkay's initial focus on jokes, there is no reason why we cannot consider even the quickest telling of a joke to be a "self contained" sphere: it will probably have a clearly defined beginning in the form of some preface, it will have a middle content,

and it will have a clearly defined ending in the form of laughter (or some substitutive activity). If this is accepted, then the import of Schutz's formulations can be realised. In comparison to Mulkay's attempts to formulate distinctions between the humorous and the serious mode - talk of differing plausibility requirements and humour as a temporary abandonment of the ordinary world - Schutz offers the more concise view that what occurs within the self contained sphere of humour is treated with an "accent of reality". That is, the jokeable is treated as having really happened, but with the upcoming transition back to "paramount reality", we know that despite the "accent of reality" given to the jokeable in the making of the joke, in reality the events did not happen.

This view bears some similarity with Mulkay's argument, but the essential point of difference is that Schutz emphasises the sequential placement of humour (or any self contained event) in respect to "paramount reality", while Mulkay strives to portray humour and the ordinary (serious) as two distinct realms each with their own interpretative processes. The point is that Mulkay's distinction between the serious and the humorous modes is an entirely unnecessary and artificial one. The import of Schutz's view is totally damning of Mulkay's argument that "In its purest form, it [humour] constitutes a radical alternative to the way in which we create our ordinary social world" (222). While humour can certainly be seen as a finite province of meaning, it is not as if it is operating against paramount reality, but in sequential relation to, and within, it. Jokeables receive an accent of reality, but to suggest that humour is a potentially liberatory force is, to use Sharrock and Anderson's

examples (64-65), like having a dream of winning a million dollars and then telling the bank manager you would like to clear your overdraft; or like informing the police that you have just seen the murder of Julius Caesar, but fortunately, you can name the murderers.

Paramount reality both antedates and postdates other provinces of meaning, including humour, and as such provides the ultimate evaluation of reality - a stable baseline. When the joke finishes we return back to the known-in-common and taken-for-granted ordinary world which as the base for humour and the evaluator of reality forms a 'seamless web'. With Mulkay, this view does not deny that problems of multiplicity, contradiction, and incongruence occur in everyday life. But, against Mulkay's view that the ordinary, serious world is a "babble of discrepant voices", Schutz's view presents paramount reality as an essentially uninteresting background, which confers a sense of stability and order because of its very taken-for-grantedness. Garfinkel's breaching demonstrations (1963) highlighted this feature of paramount reality: when Garfinkel and his experimenters 'made trouble' in the taken-for-granted realm the subjects were extremely threatened and disrupted. This showed just how crucial the assumption of a routinely structured reality was for social order. In addition, the demonstrations showed that social order is a 'serious' accomplishment: departures from routine interaction drew sanctions and morally implicative work by participants. In the face of these findings, Mulkay's argument that people face an "onerous duty of maintaining a unitary world-view" (215) seems badly misfounded. It is much more likely to be an absence of

'paramount reality' as a backdrop that makes the social world an "onerous" place.

Overall, when Mulkay's analysis of the relationship between the humorous and the serious is closely scrutinised, serious problems appear. It may be far more productive, analytically, if we move from a conceptual framework where the serious and the humorous are distinct modes of discourse, to a view where humour is done seriously within everyday organised activities. This view will be discussed more fully in the concluding discussion, but in the meantime it is kept central in the following empirical analyses. In general, the analyses exemplify the rejection of Mulkay's approach to humour, and return to Sacks' project for guidelines on how humour should be analysed sociologically.

4.4 *Analysis*

The following analysis focuses on three exhibits of 'humour' at work. Each exhibit is approached using a standardised format which consists of an examination of the following:

1. **practical activity:** the overall practical activity displayed in the exhibit.
2. **solution:** how the practical activity is achieved.
3. **endogenous order:** here, the question pursued is how humour is accomplishable using the surface materials that the exhibit provides for sense-assembly work. That is, the assumption is that humour is locally produced through a manipulation of the materials embodying it, for example lexical choice, categorisation, and sequential properties.
4. **background knowledge/import:** the 'seen but unnoticed' background knowledge which operates all along at the core of the exhibit contributing to its intelligibility. 'Import' is used in the sense of the question: what may be learnt about male (in)fertility (or other matters) from the exhibit and its analysis.

What's On

Exhibit 1. Sperm Bank Five¹¹



The Auckland band, Sperm Bank Five. The zany group will perform at the Subway, Tavern Rachel, tomorrow evening

Noteworthy

*** ** *

The Sperm Bank Five, an Auckland band whose wacky stage outfits and antics recall the legendary Tubes, will be performing at the Subway, Tavern Rachel tomorrow.

The group once performed as support for American band the Butthole Surfers dressed with horns and pink sausage costumes. At other gigs, their singer was sus-

pended from a bungy cord, sang inside a giant yellow duck, had a TV set on his head and a "muscle" suit on.

Sperm Bank Five describe their music as humorous, "death grunge" and rap. They use samples of TV advertisements and squeaky toys over a traditional bass, drums, guitar line-up.

Bassist Lindsay Fog says Sperm Bank Five "like to take ridiculous parts of ordinary, everyday life and make them seem even stupider".

The group are promoting their recently released, seven-song "Slave To Momba" cassette.

Their talents are also being tapped by MC OJ and The Rhythm Slave. Sperm Bank Five are cur-

rently creating two backing tracks for the rappers' second LP and live show.

*** ** *

¹¹ Photocopied (slightly reduced and adjusted) from an article in *The Christchurch Press*, August 23, 1991, page 27.

1. Practical Activity. Clearly, the practical activity is to provide advanced notice of Sperm Bank Five's appearance. But I want to use this exhibit for its evidence of an earlier practical activity which is carried forward and instantiated in this text: coining a 'catchy' band name (surely an activity which has had a great deal of time devoted to it since the advent of pop music).

2. Solution. The solution to the problem of coining a 'catchy' band name is to take something which normally would be treated seriously, and treat it in a somewhat frivolous, profane, contemptuous way. The band seem to adapt a common understanding of 'sperm banks' as a serious matter for their own organisational purpose -finding a 'catchy' name. Manifestly, this purpose has nothing to do with 'real' sperm banks. The exhibit is included as an example of humour, because, to take the apt phrase from the blurb which accompanies the photograph, the band is "zany" and they have found a "zany" name. The name does not invite sustained laughter, rather, it is more likely to produce an invisible smirk or stifled one-outbreath laugh - there is certainly something humorous about it.

3. Endogenous order. When we look at the picture we do not see a band performing in front of what might be the signpost for a sperm bank, one numbered five. We do see a five piece band called Sperm Bank Five: the name-as-backdrop tells us who the band is, and the five members tell us what the large emblem and words are -their name. Further, the caption to the photograph tells us that the group is "zany", and we add this to our previous sighting of the appearance of

the band and its name to look further for its zaniness. If we proceed to read the text which accompanies the photo we learn more about the zaniness of the band, including a 'quote' from one band member: "Sperm Bank Five "like to take ridiculous parts of ordinary, everyday life and make them seem even stupider". Thus, there is little problem in finding order from within the surface materials provided: the explanation for the band members' choice of name is manifestly obvious - they are doing being zany.

4. Import/Background knowledge. There is something serious in Sperm Bank Five's doing being zany, but I do not think it is the case, as Mulkay's line of argument would suggest, that the band's choice of name is contributing to a liberatory discourse on sperm banks or male infertility. The exhibit provides scenic material for how it is to be understood: the 'natural' reading is of a band doing being zany, where a particular instance of this is the use of a phrase with relatively fixed semantic qualities as a name. The fact that the words "sperm bank" are now used for a different reason and in a different context than the ordinary (serious) usage, is not a warrant for a functionalist argument connecting the two realms. Rather, in any context the use of the words "sperm bank" should be seen as a device for accomplishing local practical activities.

This said, there is some value in looking at the exhibit for its relation to the serious side of sperm banks. It can be suggested that the act of inverting the ordinarily serious to create zaniness, first involves the utilisation of 'essentially uninteresting' knowledge of what counts as a serious matter. The Sperm Bank Five exhibit offers

evidence that sperm banks can be treated in a presuppositional way as serious matters - the seriousness of sperm banks has become an unexplicated resource for the band's social act of doing zaniness. Hence, the exhibit contains a secondary elaboration on the nature of sperm banks, one which does not challenge the ordinary seriousness of discourse about sperm banks, but makes it presuppositional that they are indeed serious matters.

Exhibit 2. Woody Allen's Joke

This exhibit is an extract of dialogue from Woody Allen's 1985 movie *Hannah and Her Sisters*. A major theme of the movie is the vagaries of modern adult life, and included in the troubles that its protagonists face is infertility: Mickey (played by Woody Allen) and his partner, Hannah, are infertile and this has been attributed to male factors. The following dialogue occurs between Mickey and Hannah:

Mickey: I'm so humiliated.

Hannah: Could you have ruined yourself somehow?

Mickey: How could I ruin myself?

Hannah: I don't know. Excessive masturbation ...?

Mickey: Hey, you gonna start knocking my hobbies? Jesus!
(as transcribed in McCann, 1990: 236)

While this is an extract from continuing dialogue, the existence of a 'punch line' gives the talk a self-contained feel, and it can be treated as a unit irrespective of the preceding and following talk.

1. Practical activity. The practical activity here is to make the viewers of the movie laugh and it is probably safe to assume that the dialogue does indeed do this.

2. Solution. At a basic level the production of humour is achieved by a simple inversion: Mickey admits that he treats masturbation - an activity normally done in private and not openly talked about - as a 'hobby'. In terms of a jokeable, the humour of this strip of talk is achieved by this clear inversion.

3. Endogenous order. As noted above, humour is produced here by inverting commonsense knowledge that masturbation is, in one word, sensitive - the punch line works by a revelation that Mickey masturbates as a "hobby". But from an ethnomethodological point of view, the sensitivity of masturbation as a topic does not exist independently of talk about it; instead, the sensitive character of masturbation is constituted by ways of talking about it sensitively (see Bergmann, 1992; Silverman & Perakyla, 1990; Weijts, Houtkoop & Mullen, 1993). In very condensed form this is exactly what happens in the joke: the form of the talk leading to the punch line establishes the sensitivity of masturbation so that it can then be inverted. Looking at the joke it seems clear that the question Hannah wants to broach is whether Mickey's infertility is due to excessive masturbation - a sensitive topic. However, instead of a blunt accusation stating her suspicion, she accomplishes sensitivity by first asking "Could you have ruined yourself somehow?". "Ruined yourself" flags sensitivity because its imprecision is sufficient enough to minimise directly negative accusation. Then, in choosing to ask for

an unpackaging of the term, Mickey performs a crucial action: a favourable environment is created for the negative assessment because it is now being offered in response to an inquiry, rather than as something volunteered.¹² There is a request for Hannah to tell Mickey what he is being accused of, which by now is flagged as a sensitive topic.

The accusation does indeed follow next, but in a two part form which further enhances the production of talk about masturbation as a sensitive matter, thus making it available for inversion. The first part is the standardised utterance "I don't know" which is a perfectly legitimate answer by itself, however, once uttered it is immediately followed by an offer of a candidate explanation - "Excessive masturbation ...?". The "I don't know" appears to be doing within-turn preliminary work, that is, by first offering "I don't know" the offered candidate explanation is flagged as a sensitive one.¹³ This accomplishment of sensitivity is further enhanced by the movement to a term from a clinical register: "excessive masturbation" comes from medical-psychiatric-psychological discourse and its use in preference to folk terms (which are often used as forms of abuse, eg. 'wanking', 'wanker') gives the assessment a detached, clinical tone.

¹² Button and Casey (1985) provide empirical examples of instances where interactants introduce negative comments in a sensitive way, or talk on a sensitive topic, by an answer to an inquiry rather than as an unsolicited comment; and the joke seems to follow this common conversational device. Also see Weijs, Houtkoop and Mullen (1993) for other examples.

¹³ Tsui (1991) identifies six pragmatic functions of the utterance "I don't know" within ordinary conversation. The particular use of the utterance in the joke fits best with what Tsui calls "minimization of impolite beliefs". This refers to the situation where "I don't know" is used to preface negative assessments of the addressee, hence functioning to minimise the face-threatening effect of the negative assessment.

This carefully crafted sensitivity is, however, overturned in the punch line, where we find Mickey reformulating the candidate explanation as a direct accusation, one, moreover, which he is not happy about. This unhappiness is not because he has been accused of "excessive masturbation", but because his "hobby" has been "knocked". Hence, the humour of the joke. In addition, given that the joke involves a male making a direct complaint about sensitively packaged explanations by his spouse, it opens up the possibility that this joke is secondarily elaborative of the relations between the sexes in marital situations (the latter is a major theme of the movie). I will pick this latter point up in the next section, because it obviously seems to operate more at the level of background assumptions.

Before proceeding, given that the above discussion of this exhibit has covered diverse material, it is worth providing a brief summary of the endogenous order of Woody Allen's joke. The argument is that the endogenous order is crucial to the joke's success in accomplishing humour. First, it is accomplished in an economical way through the use of question-answer adjacency pairs - questions require answers and are therefore economical devices for providing the material for humour. Second, questions can be used in a way which accomplishes the sensitivity of a topic: in this case "excessive masturbation" emerges as a candidate explanation through a request rather than a volunteering. Third, the candidate explanation is prefaced with the utterance "I don't know" which effectively indicates that the explanation's producer offers it with due respect to how the recipient may hear it. All this is crucial to the final punch line. For, while at a content level the joke's humour is produced by an admission that

masturbation is Mickey's hobby, that is, a clear inversion of the sensitive manner in which masturbation is discussed in our culture, the sensitivity of talk about masturbation is displayed incarnately in the joke's formal structure, thus making it available for humorous incongruity. The joke's form and content are inseparable elements in its successful accomplishment of humour.

3. Background knowledge/Import. The exhibit is a useful one for pursuing Mulkay's argument that humour is a potentially liberatory force in society. As noted above, "excessive masturbation" is a term from the clinical register of medical-psychiatric-psychological practice, where it has been identified as a possible causal factor for male infertility.¹⁴ The exhibit also shows the use of a more general and lay concept for the same causal knowledge, ("ruining oneself"), and given the presence of both these aspects it is clear that the humour is grounded in real world discourse about masturbation as a possible cause of infertility. In other words, from Mulkay's point of view we have some referral back to the world of serious discourse about real events. Following from this it could be argued that because the joke inverts norms about masturbation, it could form a small part of resistance to serious discourse about masturbation and the way such discourse might constrain people. Sketchiness aside, there is a degree of plausibility to such an argument.

¹⁴ For example, one of the more comprehensive medical textbooks on infertility states that "Sexual abuse through excessive masturbation with pollution or abuse of the sexual drive leading to or associated with sexual psychopathology may be accompanied by infertility" (Joel, 1971: 74).

However, I would like to offer a different view, as follows. If, in any situation we take a view of the human actor as an inquirer into social affairs (Sharrock & Anderson, 1982), rather than an expert on social affairs, then this exhibit can be approached not for how it can be used as ammunition to break down existing ways things are known or talked about, but for how it makes knowledge of a culture available to that culture's members. In this instance, and this is the import of the exhibit, it makes available for us the knowledge that within heterosexual marital unions, which provide a normative-legitimate site for stable sexual relations, masturbation by the male partner may be a 'normal' and ordinary state. However detailed private knowledge of such events may have been, in the joke as a social event it is now made available as common cultural knowledge, albeit at a secondary level. Somewhat speculatively, I might also suggest that Woody Allen's joke tells us something about the character of this practice: it is a sensitive topic within relationships. While, on one hand, Mickey knocks down the sensitivity with which Hannah so carefully constructs her candidate explanation, on the other hand, he reinstates sensitivity at another level: he shows men being sensitive about the issue of their private sexual practices within stable heterosexual unions. The humour lies in the punch line as a complaint, but for all that it is a complaint about real world practices, which are now made available to members as inquirers into their own culture.

Exhibit 3 The Garp Extract and Joan's Story

Two pieces of data make up this exhibit. The first is an extract from John Irving's (1976) novel *The World According to Garp*. The extract covers the story of how Garp's mother, Jenny - a nurse in a hospital - went about conceiving Garp. The whole story is interesting, but unfortunately it occurs over several pages and so I have summarised sections of it, while trying to keep its sequentiality intact. Here is the extract:

Jenny *told* the other nurses that she would one day find a man to make her pregnant - just that, and nothing more. She did not entertain the possibility that the man would need to try more than once, she told them. They, of course, couldn't wait to tell everyone they knew. It was not long before Jenny had several proposals. She had to make a sudden decision: she could retreat, ashamed that her secret was out; or she could be brazen.

A young medical student told her he would volunteer on the condition that he could have at least six chances over a three-day weekend. Jenny told him that he obviously lacked confidence; she wanted a child who would be more secure than that.

An anesthesiologist told her he would even pay for the baby's education - through college - but Jenny told him that his eyes were too close together and his teeth were poorly formed; she would not saddle her would-be child with such handicaps.

One of the other nurses' boyfriends treated her most cruelly; he frightened her in the hospital cafeteria by handing her a milk glass nearly full of a cloudy, viscous substance.

"Sperm," he said, nodding at the glass. "All that's *one* shot - I don't mess around. If one chance is all anyone gets, I'm your man." Jenny held up the horrid glass and inspected it coolly. God knows what was actually in the glass. The nurse's boyfriend said, "That's just an indication of what kind of stuff I've got. Lots of seeds," he added, grinning. Jenny dumped the contents of the glass into a potted plant.

... [we then learn that because of Jenny's attitude she is shifted to a new section of the hospital -intensive care. This is mainly

full of soldiers, whose non-accidental injuries are categorised by Jenny as 1. Externals, 2. Vital Organs, and ...]

3. There were the men whose injuries seemed almost mystical, to Jenny; they were men who weren't "there" anymore, whose heads or spines had been tampered with. Sometimes they were paralyzed, sometimes they were merely vague. Jenny called them the Absentees. Occasionally, one of the Absentees had External or Vital Organ damage as well; all the hospital had a name for them.

4. They were Goners.

"My father," Garp wrote, "was a Goner. From my mother's point of view, that must have made him very attractive. No strings attached."

... [We are then given a description of how Garp senior sustained his injury, and then we see Jenny taking him under her care ...]

Given the evidence, the shadows, and the white needles in the X rays, Gunner Garp was probably a Goner. But to Jenny Fields he looked very nice. A small, neat man, the former ball turret gunner was as innocent and straightforward in his demands as a two-year old.

... [Subsequently, Garp regresses to the state of a baby. Jenny recognises this and gives him the comforts that babies need, to the extent of suckling him at her breast; amidst this Jenny's wish to have a baby still persists and is stimulated by Garp's nursing ...]

... Her feelings were so vivid - she believed for a while that she could possibly *conceive* a child by simply suckling the baby ball turret gunner.

It was almost like that. But Gunner Garp was not *all* baby. One night, when he nursed at her, Jenny noticed he had an erection that lifted the sheet ... And so one night she helped him; with her cool, powdered hand she took hold of him. ...

When he came, she felt his shot wet and hot in her hand. Under the sheet it smelled like a greenhouse in summer, absurdly fertile, growth gotten out of hand. You could plant *anything* there and it would blossom. Garp's sperm struck Jenny Fields that way: if you spilled a little in a greenhouse, *babies* would sprout out of the dirt.

Jenny gave the matter twenty-four hours of thought.

... [And the next night she copulates with Garp, for the one and only time, confident " that the magic had worked"]

Thus was the world given T.S. Garp: born from a good nurse with a will of her own, and the seed of a ball turret gunner - his last shot. (from pp. 13-24, emphasis in original)

The second part of exhibit three comes from an interview that I did with a technician whose work within a donor insemination unit involved managing the sperm bank. This particular piece of talk came in response to my questions about what she told people about her work:

(H9: Joan)

- M: Are there any things that you can actually remember that have been said?
- J: To me?
- M: Yes
- J: About it, jokes made?
- M: Yes
- J: Oh yeh, someone, we were out for dinner for our netball, someone tossed over, it was said quite loudly, someone said to someone else 'did you know what Jackie does?'. And they said 'no'. They said that she does the sperm bank, and someone tossed me over a, an inner out of a wine cask, and said, 'here, I've filled this, it's yours', you know. I thought 'oh, okay'. And they said 'you do not' and I said 'I do', and they said 'you don't', I said 'I do', they didn't believe me at all ...

I will call the first part of this exhibit 'the Garp extract' and the second part 'Joan's story'. While there is obviously a lot more to the Garp extract than Joan's story, I have put these two extracts together because, amongst other things, they both use the activity of a man handing a 'sperm' sample to a woman to produce humour. I will analyse these together using my previous format; the analysis will

focus mostly on the Garp extract because firstly, it provides more data for analysis, and secondly, because an understanding of Joan's story flows nicely from an understanding of the Garp extract.

1. Practical activity. The Garp extract is part of a humorous novel which has the antagonistic relationship between the sexes as a central theme. Thus, the overall concern of the novel is to accomplish humour, and this is done in an incremental way by providing individual instances of humour, which are very evident in the Garp extract. But the other side to the novel is that its humour occurs within the background theme of the relation between the sexes - a perennial and serious topic.

Joan's story provides a very brief account of some (presumably) real world events, and I think that these events are an example of what most people would recognise as a 'practical joke'. It seems clear that we have a perpetrator (the male 'filling' the inner with 'sperm'), a 'fall-guy' or victim (Joan), and an audience (the others at the dinner who see or hear the event). The practical activity is to make the audience laugh using the events foisted upon the 'fall-guy'. It appears from the Joan's comments that she did not laugh but accepted the events in a somewhat neutral manner, while we can only assume that some laughter eventuated in the audience.

2. Solution. The Garp extract and Joan's story contain two very similar events. In the Garp extract a milk glass nearly full of sperm is handed to Jenny as proof of suitability for the part of 'one-shot' candidate. This action is sensibly labelled in the extract as a "cruel

joke". That is, it achieves humour by making fun of something that somebody takes seriously. In this case Jenny's intention to have a child, and in one attempt, is serious; the 'glass-filler' takes this serious intent and makes public fun of it by exaggeration - he suggests the filled glass is the result of one ejaculation. The same seems to apply in Joan's story, but this time it is a wine-cask inner that is handed over. The humour is accomplished by treating something which is serious in an exaggerated fashion.

3. Endogenous Order. There are difficulties in analysing Joan's story as it is obviously a retelling of real events, but we can make the very simple observation that the 'practical joke' has a basic temporal grounding in that a first part is the serious disclosure by Joan that she works in a sperm bank, and this then becomes the base for the offering of a 'filled' inner as a practical joke. The Garp extract offers greater potential for analysis and we can see in it a more complicated sequential base to its order (here I am referring to the whole extract and not just the 'practical joke' part).

The story begins with Jenny telling the other nurses that "she would one day find a man to make her pregnant - just that and nothing more".¹⁵ Further, Jenny "did not entertain the possibility that the man would need to try more than once"; and it is this belief that sets up a very effective humorous play on words. First, a candidate offers to do the job but on the condition that he can have six chances - it is clear that he 'wants a shot', but once is not enough. Jenny declines

¹⁵ It should be added here that it has been established previously in the book that Jenny dislikes the physical act of sex, or as she calls it "peter treatment", and this is well known amongst the hospital staff, who subsequently treat her as an oddity.

this offer with reference to the seriousness of her endeavour - a man who needed more than one chance was not suitable. Next, in a somewhat more serious vein, an anesthesiologist offers but is turned down on the grounds of his physical appearance. The next offer is what I am suggesting is a practical joke: in a public space Jenny is handed a glass, apparently full of sperm, with the male saying that if one chance (shot) is all anyone gets then his "one shot" is evidence that he is the man for the job.

Note the economy with which the practical joke is achieved. The glass filling incident is preceded by two men trying to get a shot, but being turned down by Jenny. Thus we have a sequential environment where a third action is related to the previous two but can be different in important ways (see Sacks, 1978). It can be suggested that given the prior offers being turned down, the action of the 'glass filler' is rendered as an action on behalf of men who are being bettered by the woman, hence, its availability as a 'practical joke'. The offer of a glass filled with 'sperm' is naturally framed within the prior rejections thus giving it meaning beyond its immediate environment.

But of course, Jenny is not upset by the practical joke and triumphs again. She dumps the glass of sperm in a potted plant and then finds the perfect candidate - a "Goner". That is, a man who cannot display the typically masculine intent of the previous candidates: intent 1 (medical student), to have sex; intent 2 (anesthesiologist), probably the same, but unsure; intent 3 (nurse's boyfriend) to have a shot at Jenny, in both senses of the term. As a 'goner' Garp cannot make a negative 'shot' at Jenny, only a procreative 'shot', hence, the wonderful

culmination of the play on words: Jenny gets her one shot (at procreation), which she is sure is enough, and ball turret gunner Garp gets his last shot.

These are the embodied materials with which the humour is produced. They are organised in a very common structural form which is more than sufficient to produce multi-layered humour. It is multi-layered because while it produces humour it is also recognisably 'saying' something about the relations between the sexes: it is a tale in miniature of a woman attempting to get something she wants within a male system, and in this quest we see her coming up against typically male motives and intentions. The only way she finally wins is by finding a male without motives and intentions, but with the biological capacity to reproduce. But, again this secondary aspect of the humour is also made available within, and from, the incarnate order of the extract: we find this other meaning within the exhibit's order and not primarily by bringing exogenous factors to it.

4. Background Knowledge/Import. A particularly important part of the background knowledge used to accomplish the humour in exhibit three stems from membership categorisation. Briefly, typifications of masculinity are central to the exhibit, and provide crucial links to the overall theme of Irving's novel, that is, antagonistic relations between the sexes. In the practical joke's development we deal first with a candidate who is categorised as a "young medical student". We find that he will take up Jenny's quest if he can have six chances over a three day weekend. The key word here is "young": we have no difficulty in inferring that the appropriate category bound activity for

a young male is to be on the search for sex, and as frequently as possible. The second candidate is described as an "anesthesiologist" and he tells Jenny that "he would even pay for the baby's education - through college". Thus, the essential activity that goes with being an anesthesiologist is being able to pay for a college education. We are told little else of this candidate, except that he is rejected because of his appearance; and in a way he seems unimportant except for the structural reason of 'making up the numbers' for the pattern of two rejections (victory for Jenny), followed by an attempt by men to get back at her via the practical joke.

When we get to the practical joke, the practical joker is categorised as "one of the other nurse's boyfriends". This is what Sacks (1974) calls a "duplicatively organised" MCD, that is, it involves a pair that goes together, in this case girlfriend and boyfriend. Crucially, a natural inference about boyfriend-girlfriend pairs is that such a unit is a place to find sexual activity. Sexual activity is a category bound activity for someone described as 'boyfriend' or 'girlfriend'; this may be an inference of full fledged adult sexual competency, but at the least it is of sexual learning and familiarisation. I believe that this categorisation is crucial for the practical joke that this male plays on Jenny. Given the categorisation of him, we are likely to see a sexually competent male handing over a putative sperm sample to Jenny, and not an inexperienced boy. In this way the male is available as one who can play a practical joke by the display of a glass full of a "viscous substance", and without this base of sexual authority the practical joke very likely would not work. The use of a "filled glass" of sperm for a joke is strongly connected to the

recognition of sexual competence. If, for example, we substitute the categorisation "young medical student" for "one of the other nurse's boyfriends" the practical joke seems less natural - it just does not seem an appropriate activity for a "young medical student". For the practical joke to work, an important activity that the employed categorisation must accomplish is a male "doing masculinity"¹⁶ of an adult, sexually competent kind.¹⁷

In many ways, the Garp extract is a literary sociological analysis of "doing masculinity". The description of Garp senior is the climax of this analysis. While he possesses male genitalia, he cannot do masculinity as an activity-bound display. He is a "Goner", and then a goner who regresses back to infancy. But, he still possesses what is needed for reproduction: he is, if you like, a baby with an erection, and moreover, his sperm is assessed by Jenny as "absurdly fertile". This is interesting, because both the practical joker and Garp have a sample of their sperm available for Jenny's assessment, in fact, one is in a glass and one is "in her hand", but the former is assessed by Jenny as "horrid" and is dumped into some soil, while the latter is assessed not for what it is, but for its potential - "You could plant *anything* there and it would blossom". The essential difference between the two candidates, which has been skilfully built up all

¹⁶ For a detailed discussion of the notion of "doing masculinity, or "doing gender", see West and Zimmerman (1987).

¹⁷ Somewhat speculatively, there is a possible extrapolation to Joan's story. One could hypothesise that in Joan's story the male offering the 'filled' wine inner would not be a sexual learner, for this would be to risk having the joke turned upon him. Just as with the 'glass filler' in the Garp extract we can reasonably suggest that as well as accomplishing a practical joke, the male offering the filled inner is doing masculinity, and again this is particularly bound up with the activity of sexual competence.

along in the text, is that one has typical male motives and intentions while the other has none - one does masculinity and the other cannot. It is the one who cannot do masculinity whose sperm is assessed positively by Jenny, and it is he who gets his 'shot' at Jenny, except now Jenny is 'on top'.

In this very artful mixing of incongruity and inversion humour abounds, but so also does serious comment on male-female relations, in specific, troublesome male motives and intentions. Overall, we see in exhibit three, as in all the exhibits, members doing humour, seriously. An explication of the phrase 'doing humour, seriously' brings us full circle to the work of Sacks, and this will be discussed briefly in the concluding discussion, which now follows.

4.5 *Concluding Discussion*

It should now be apparent why this chapter began with an explication and critique of Mulkay's *On Humour*. In a sense, to understand the import of treating humour as a procedurally structured activity that is done seriously, one needs to juxtapose such a treatment with alternative and, at the moment, dominant sociological treatments.¹⁸ Mulkay's work happens to be a very recent and in-depth treatment of humour, which also utilises ethnomethodological research, hence, its choice as a foil. It should be clear both from the

¹⁸ For another recent, detailed sociological study of humour see Zijderfeld (1983). Zijderfeld makes a very similar argument to Mulkay, again using Schutzian concepts to argue that humour establishes alternative realities.

critique and the analyses above, that I do not find much appeal in Mulkay's approach.

To summarise, I argued firstly that Mulkay's recourse to "plausibility requirements" to understand humour was unnecessary. The concept displays a clear tendency to separate thought and language-in-use, consequently, its analytical value is doubtful. The concept bears an unknown relationship to actual instances of humour in action, and, in logical terms it leads to an infinite regress. If these arguments are valid, then there are good grounds for dispensing with the concept of differing plausibility requirements in the analysis of humour. Secondly, I considered the general functionalist theme in Mulkay's argument. That is, he suggested that the serious social world contained a fundamental contradiction: people assume a unitary real world, but actually experience vastly different lives, which makes social life a "potential babble of discrepant voices". Making a functionalist step, Mulkay argued that humour could be used both, as an 'escape attempt' from the serious world and its "onerous" duties, and, as a potential point of resistance to serious social issues. Via the arguments of Sharrock and Anderson, I argued that there was no warrant in Schutz's work for a view of individuals as ego-centred formulators of competing multiple realities. Rather, the import of Schutz's view was to focus on the essentially sequential nature of the everyday world: despite different experiences and viewpoints there is always a taken-for-granted baseline position to return to. Ethnomethodological work has long suggested that the assumption of a unitary real world, far from being "onerous", could actually be of

crucial importance to social order and the very possibility of members being 'at home' in their intersubjective world.

Following from the explication and critique of Mulkay, I offered some analyses of humorous discourse, where that discourse was in some way related to male (in)fertility. In contrast to Mulkay's approach, the aim was to show that humour is through and through a matter of local production. Thus, it was argued that 'Sperm Bank Five' as a band's name was available as an aspect of them doing being zany, and not as a putative comment on the real world activity of sperm banks. However, in addition, I argued that the exhibit did provide evidence for the existence of a presupposition that sperm banks are indeed serious business. In the Woody Allen Joke we saw an interesting manipulation of the sequential properties of talk: the joke 'worked' by inverting norms about masturbation being a sensitive matter, but given this, the first task the joke had to accomplish was a display of the very sensitivity of talk about masturbation. This was accomplished very simply and economically by a variety of conversational devices, devices which because of their utter familiarity remained 'seen but unnoticed'. At another level I argued that the joke, coming as it did within a movie concerned with male-female relations, contained embodied materials for finding, within a heterosexual union, conflict over the private sexual practices of the male. It was not as if this provided a counter discourse to be used in revolt against constraining societal norms, values, and so on; rather, it was an important piece of knowledge now made available to members as inquirers into their own culture. Similarly, the analysis of the third exhibit showed how humour was accomplished using common

procedural and structuring mechanisms. These also embodied the 'serious' aspects of the humorous activity, in this case the "doing of masculinity" within the context of male-female antagonisms.

In contrast to Mulkay, the analyses made no appeal to modes of discourse, plausibility requirements, nor to interpretative procedures where these are framed as a kind of information processing. Instead, the emphasis was on analysing the formal orderliness of humour, where that orderliness is produced by and within the very structure of humour. In this regard, we can see the imprint of Sacks' mode of analysis, and it is to this that I now turn to conclude.

Recall my gloss of Sacks: he argued that the structure of dirty jokes hid away implausibilities in the chain of events utilised to produce humour. As outlined, it was this comment that Mulkay took issue with and led him to introduce "plausibility requirements". But Mulkay failed to appreciate that Sacks' analysis of the dirty joke is, like all his work, strongly based on the ethnomethodological principle of "relevance".¹⁹ His main insight here was to look at relevance in terms of "sequential relevance". For example, in "Everyone has to lie" (1975) he critiqued the then prevalent abstract philosophical discussions of true-false contrast classes, by neatly showing how the use of particular utterances sequentially relevanced the production of a true or false assignation. Similarly, in regards to humour, the simple point is made that an offer to tell a joke relevances the production of laughter or some sign of having 'got the joke', and not a

¹⁹ See Schegloff (1991) for a concise discussion of the principle of relevance.

true/false assignation. This is the context in which Sacks made his remarks about jokes necessarily hiding away implausibilities.

The general point to be taken from Sacks, is that the initial prefacing work which sets up the telling of a joke, makes the assignation of true/false (or plausible/implausible) irrelevant, but this is something that must be maintained throughout the remainder of the joke's telling. In social interaction any first action is subject to misunderstanding, ambiguity, mishearing and concomitant repair work, hence the social accomplishment of laughter from a joke is a concerted, constrained, dynamic matter. Given this we can understand why Sacks commented that the dirty joke hid away implausibilities: the joke has an organisational task to accomplish - laughter - and any introduction of non-essential items would detract from the economical accomplishment of that task. Therefore, Mulkay's argument that in the humorous mode speakers "are allowed much greater freedom in what they can say" (20), is perhaps only true in terms of the jokeable -impossibilities can be talked of - but, in terms of the joke as an interactional accomplishment, it is possibly more tightly constrained than so called serious discourse.

It is the dynamic and organisational nature of interaction that provides the rationale for moving from talk of the serious and the humorous, to talk of doing humour, seriously. Humour is a serious social accomplishment, just as serious, that is, as the matter of establishing the orderly properties of any situated conduct:

The demonstrably rational properties of indexical expressions and indexical actions is an ongoing achievement of the

organized activities of everyday life. Here is the heart of the matter. The managed production of this phenomenon in every aspect, from every perspective, and in every stage retains the character for members of **serious, practical tasks**, subject to every exigency of organizationally situated conduct. (Garfinkel, 1967: 34, emphasis added)

In this sense there is no escape from the serious practicality of everyday life.

Having collapsed the distinction between the serious and the humorous in favour of a view where all social activities are serious accomplishments, there is still a sense in which we can see humour involved in activities that are manifestly non-humorous. Why not retain the reasonable notion that some things are funny and some things are not, but avoid stating this in terms of bi-polar modes of discourse. For example, Woody Allen's joke is funny, but many of the real world activities that it encapsulates are manifestly not: conflict over private sexual activity within a stable heterosexual union is, potentially, a serious problem. However, this is not a warrant to view humour as a "potentially liberatory force". I prefer the view that humorous discourse is, amongst other things, available for interrogation by members for the 'serious' information it contains about their culture. That is, we need not work with a view where culture, society, social reality or social structures are stable because of the interrelationships between structural sub-elements - in Mulkay's case humour as a distinct mode of discourse functioning to patch-up the paradoxes of serious discourse. Instead, following Sacks, we could maintain that a culture "... is stable by reference to everybody seeing the world for what it is, without regard to whether it's pleasant [or humorous] or not ..." (1979: 14). The important point

is, that making the world available for what it is, is no trivial achievement. To use a felicitous phrase from Sacks (1974), it requires the massive presence of the "fine power of a culture".

Chapter Five

The Relationship Between Social Science and Common-sense, Analysed with Respect to Accounts of Non-response

5.1 *Introduction*

The vast majority of research on infertility has focused on infertility within heterosexual couples. Following from this, an obvious research question has concerned gender differences in response to infertility: do men and women within infertile couples vary in their response to infertility?; in what general ways does male infertility differ from female infertility? Research on this issue has produced an apparent consensus on two points. First, it is claimed that men and women do react differently to infertility, with women finding it more of a problem than men. Recently, Greil, Leitko and Porter (1988) reviewed the literature on this issue and stated that "These studies will not support the weight of a conclusive interpretation but, taken as a whole, they do seem to suggest that, just as his marriage is better than hers (Bernard 1972), so seemingly is his infertility better than hers" (1988: 180). They illustrate this with extracts from their own interview data, and suggest that while women were often devastated by infertility, men were merely disappointed.

However, as Greil, Leitko and Porter suggest, this is not a "conclusive interpretation", and the second point of apparent consensus indicates why. Somewhat less frequently, researchers have reported the comments of infertile people and others, suggesting that societal norms and values connect male fertility with male virility, sexual competency, potency, and so on. Here is an early and typical statement of this argument:

Whilst a sense of failure may be common to both sexes, it is only the proud male who regards it [infertility] as an affront to his sexual capacity. For him procreation has always served as a means of demonstrating his virility, whereas it is well known that a woman's fertility gives no indication of her sexual responsiveness. And no matter how bravely he has accepted the discovery at a conscious level, unconsciously the equation with impotence is likely to remain. (Humphrey, 1969: 52)

Supposedly, it is in this linkage between fertility and virility¹ that infertility may have the potential to be a serious problem for men.

Thus, the infertility literature is equivocal on this aspect of gender's role in infertility: on one hand, men are merely disappointed by infertility, whilst on the other hand, they may find it seriously threatening due to the fertility-virility linkage. This apparent situation of ambivalence may be partly due to the small size of the literature on this topic (for more reviews see, Abbey, Andrews & Halman, 1991; Berg, Wilson & Weingartner, 1991), and the fact that infertility research in general is predominantly focused on female infertility (see Matthews & Matthews, 1986b).

¹ Hereafter, this will be referred to as the 'fertility-virility linkage'.

This latter point could reflect a number of factors, but for certain it is connected to the difficulties researchers have experienced in gaining male respondents in studies of infertility. It is this point, rather than the equivocality of the literature, that is the focus of this chapter. Specifically, the chapter analyses the way that researchers have accounted for high rates of non-response by infertile men. This topic came about as a result of pragmatic factors: in the course of reading research on infertility it was obvious that whenever researchers attempted to study infertility they had far more difficulty in gaining male respondents than female respondents; and I also experienced this difficulty in attempting to gain infertile male respondents for this study. The upshot of both these factors is that I formed a collection of accounts of non-response in studies of infertility.

In this chapter, these accounts are approached chiefly through ethnomethodology's longstanding concern with background assumptions, or more generally, common-sense.² In short, it is argued that common-sense is central to researchers' accounts of the high rate of non-response by men in studies of infertility. While, in principle, an analytic concern with common-sense could be applied to any substantive area, the problem of non-response turns out to be a very useful point of application. It functions almost as a breaching demonstration, as the accounts of non-response display and reinforce

² No definition of common-sense will be provided here. Just what is being referred to will become clear in the following discussion, besides which, the term is mostly self-explicating: common-sense tells us what common-sense is. While aware that common-sense can be both noun and adjective, and this is partly why different presentations like 'commonsense' and 'common sense' have arisen, the term 'common-sense' will be used consistently throughout this chapter. Simply, this seems a useful compromise.

much of the orderly features of what researchers have said about infertility based on data gathered from those who will talk to researchers. As will be seen, this includes reference to the fertility-virility linkage and the supposed different impact of infertility upon men and women.

In the main part of the chapter, extracts from the infertility literature are presented as a data corpus, the accounts contained are described, and then analysis focuses on the intertwining of common-sense and social science reasoning in the accounting process. Before this empirical section a brief outline of the ethnomethodological approach to the relationship between social science and common-sense is offered. Between this outline and the presentation of the data corpus, some brief details of my own experience of gaining infertile male respondents is offered, chiefly to provide a graphic illustration of the problem of non-response, but also to make some initial points about the relationship between social science and common-sense.

5.2 *Approaching the Relationship Between Common-sense and Social Science*

As a general argument, there is nothing new in suggesting that any piece of social action, including social science practice, is necessarily based upon a whole range of common-sense background assumptions. This is one of ethnomethodology's foundational points.³ However,

³ Any introductory text or review article on ethnomethodology will provide discussion of 'common-sense'. Heritage (1984, ch.3) provides a particularly good discussion which shows the centrality of Schutz's work to the formation of

while not a new argument for ethnomethodologists, the issue of the relation between common-sense and social science has drawn "much agonising" (Sharrock & Anderson, 1991: 56) from other sociologists. Therefore, it is worth outlining the ethnomethodological position on this issue, with the aim of showing that there is really no need for agonising.

There is no reason for agonising because in its general form the ethnomethodological argument appears incontrovertible. The argument is a form of the 'always and already social' thesis which proceeds as follows. Social scientists operate by taking cultural members' categories, conceptions and theories and reformulating them in terms of general social structural principles which transcend their everyday base in common-sense. Thus, we have a transition from members' perspectives where 'things are as they seem', to sociologists' perspectives where 'things are not always as they seem'. Rather than adopt the traditional sociological motto that 'things are not always as they seem', from the beginning ethnomethodology has been concerned with the question, 'how exactly are things for everyday cultural members', or, as Sharrock and Button put it, ethnomethodology's recurrent question is "how, within a social scene, are explanations of persons' actions found?" (1991: 165). A consequence of such questioning is not so much a wholesale critique of other forms of

ethnomethodology's concern with common-sense. As well as making a general argument about the relation between commonsense and science, ethnomethodologists have now branched into the study of science itself, which is approached as a practically accomplished ordinary activity - Lynch, Livingston and Garfinkel (1983) provide a review of the ethnomethodological work in this field.

sociology, but certainly the presentation of some nagging problems for traditional sociology and its motto.

One of the more persistent of these problems has been well captured by Sharrock and Button:

It is this: show that you can collect a corpus of data which is comprised of instances of 'social action' without presupposing the organisation of a 'social structure' in the assembly of that very collection. (1991: 158, original emphasis)

The abstractness of this statement is clarified by the more detailed example that Sharrock and Button discuss. Taking education (but one could apply this to any topic area), sociologists set out to study what goes on in classrooms, and in this endeavour experience no difficulty whatsoever in finding classrooms to study. Equally, they have no difficulty in applying the categories 'teacher' and 'pupil' to the people in the classroom. The sociologist of education may choose to write up his or her research in the classroom in terms of class, gender, or ethnicity based inequalities, and must work to do so; but, it is the case that there is no difficulty in finding events-in-a-classroom. That is, this ordering work is seldom reflected upon - the categories 'classroom', 'teacher' and 'pupil' do not have to be derived from observation, but are

in place from the very beginning. They are, of course, in place from the very beginning because they are *institutionalised* (so to speak) in the social setting that is being described, because they are *socially sanctioned ways of describing events* which take place in that setting. (Sharrock & Button, 1991: 159, original emphasis)

In short, to recall a well known phrase of Schutz's, social scientists produce 'second order constructs', "namely, constructs of the constructs made by the actors on the social scene" (1962: 6). The point is that the original typification work is not pre-social but is another social accomplishment now thoroughly incorporated into the realm of common-sense. Or in other words, it is 'naturalised', now part of the natural order of things, and researchers rely on these naturalised accomplishments in their research activities.

Put in these terms it seems incontrovertible that social science cannot but rely upon common-sense, and there is no possibility of any absolute separation of the two. This is now an 'old' argument, but it has 'new' repercussions: it suggests that the particularities of the relationship between social science and common-sense is an important research topic. Sociologists and others have certainly reflected upon the role of knowledge in society, as the existence of a 'sociology of knowledge' and a growing band of sociologists influenced by Foucault testifies. However, in general these considerations have taken the traditional sociological approach of analysing how bodies of knowledge bear the marks of society, or, asking what function(s) bodies of knowledge fulfil in society. While legitimate modes of inquiry, these sociological approaches often fail to give detailed attention to empirical materials which embody the relationship between social science and common-sense.

Sharrock (1989) has noted these points above, and suggested that an alternative approach would be to study

...how professional sociological studies (theoretical and empirical) are themselves regulated by insistence upon socially sanctioned bona fide facts of life (which is another way of saying 'common sense understandings'). This is not a question to which one can turn to other sociologists for any kind of answer, for ... The role of 'the obvious', of what 'goes without saying', 'stands beyond question', or 'can be supplied by the hearer/reader' is not something that has been systematically and self-consciously treated in the main sociological tradition ... Obviously, the interest would not be in producing a sweeping characterisation of the relationship between 'sociology' and 'common sense' but in inspecting, rather, the intricate ways in which, in particular studies, distinctively sociological conceptions are inevitably intertwined with a dense body of matters that can be taken to be 'known in common' prior to the institution of the inquiry in hand and which can, as grounds for further action and inference, shape its course. (1989: 662)

This chapter makes a small contribution to this task. As noted, the particular studies that I focus upon are those where a male reluctance to partake in research on infertility has been described and accounted for. As such it should be clear that the intention is not to make a sweeping statement about the relationship between sociology and common-sense, but to learn something about accounts of infertility as substantive matters, and how the relationship between common-sense and social science is involved in these accounts.

One further proviso should also be made. As Lynch (1991) argues, while it is characteristic of ethnomethodological studies that they exercise "indifference" (Garfinkel & Sacks, 1970) about what they study, it must be emphasised that indifference is not equivalent to denial. Following Lynch, there is no implication in the following discussion that "social scientists' measurements have no more than a 'common-sense' basis or that there are no distinctions to be drawn between sociologists', coroners', physicists' or any other lay or

professional efforts to measure phenomena" (1991: 86). The point is to investigate the differences, similarities, incorporations, modifications between, with, and of, common-sense and social science. That is, to treat the relationship between social science and common-sense as a topic of investigation and not suggest that, either, common-sense is faulty, or, that social science is merely a fancified version of common-sense.

5.3 *Non-response in Studies of Men and Infertility: A First Illustration*

Social science research very often involves researchers talking to a sample from a target population ('subjects', 'respondents', 'interviewees', etc.) at some stage in the research process. While both the world of the researchers and the world of the target population are very complex, the linkage between the two is achieved by very mundane means: a communication whereby the researcher requests the participation of the subject in the research. While such communication may take many forms, including face-to-face requests, requests by telephone, requests posted in newspapers, and so on, in general these communicative acts can be considered as request-response adjacency pairs. Obviously, the 'preference' operating in this pair is 'yes', that is, the respondent indicates a willingness to respond to the researcher's questions.

But clearly, 'preference' is not akin to an invariant rule, and certainly, in the case of attempts to talk to men in studies of

infertility, researchers have often received many negative replies from candidate respondents. Below, my own attempt to interview infertile men will be outlined to provide a graphic example of this high non-response rate.

It was never the intention in this study to attempt to gain a large sample from the population of infertile men. Rather, the aim was for about thirty respondents, and after appeals in multiple sources over a period of ten months, nineteen interviews were successfully completed. The sources included: personal acquaintances and 'friends of friends'; snowball from previous respondents; requests at two regional infertility support groups and the national infertility society; contacts from social workers working with the infertile; and requests in a university newsletter. As well as the men that agreed to talk to me there were a number of refusals and other cases, and these are summarised in Table 5.1.

Table 5.1

**Results of Attempted Enrolment
in a Study of Male Infertility**

Result	Number
Candidate agreed to interview	19
Candidate directly refused	10
Refusal evidenced by candidate failing to contact	8
Candidate identified by intermediary but intermediary did not ask	5
Candidate identified but could not be asked due to inability to trace, logistical problems	5
Possible identification of candidate by intermediary, said would ask, but no feedback	4
Mistaken identification of male infertility	4
Other*	3

*Offers to participate from: a male who had a vasectomy and was not trying to have children; a male whose partner was infertile; and one partner who was infertile said her fertile husband would be willing to participate.

The table shows ten cases of direct refusals to be interviewed and eight cases where refusals can be imputed by failure to contact, that is, the men were asked if they would partake but there was no reply from them. It seems reasonable to add these two categories to constitute eighteen cases of non-response.⁴ Given that nineteen men agreed to be interviewed, a non-response rate of 48.6 per cent can be calculated. There is also a good possibility that the remaining cases include non-response, but even as is, the non-response rate represents virtually one refusal for every agreement to be interviewed. Without referring to manuals of social research methods,⁵ it is reasonable to assert that such a high non-response rate is an 'observable-reportable' matter for researchers, that is, one that naturally calls for accounting work.

Two further points can be made from the table. The first concerns the complexity of the process of gaining respondents. Apart from agreements, direct refusals and assumed refusals, the table shows a large number of additional events: in attempts to gain respondents, candidates are identified by intermediaries but the intermediary does not subsequently ask the candidate to participate; intermediaries

⁴ Note here that no attempt has been made to define 'non-response'. This is unnecessary, as the interest here is not in operationalising the concept, but in the accounting work that it stimulates. Having said this it should be clear that the way I have used the term to summarise my own attempts to gain respondents incorporates two potentially distinct categories. That is, those who directly refuse, and those who are asked to participate by the researcher or an intermediary, but who do not contact the researcher about the request to participate. It seems a fair assumption to group these together as 'non-response'.

⁵ If one was interested in social science comment on non-response and related issues the journal *The Public Opinion Quarterly* provides ample and regular discussion.

mistakenly identify candidates; and people mistakenly identify themselves as candidates. Hence, the apparently simple task of setting out to interview people of category 'X', where it is the researcher who has operationalised 'X', comes up against common-sense notions of what is meant by that category, and non-correspondence between the two is not uncommon. It is likely that such non-correspondence is generally left out of research reports where precise-looking figures of response and non-response rates are calculated, but common-sense decisions are undoubtedly involved in the production of these figures.

A brief elaboration shows this process in more detail. In my enrolment attempt, the vast majority of candidates were identified by intermediaries, and often for various reasons including pragmatics and propriety, it was the intermediary who contacted the candidate to ask if they would participate in my study. I would then check with the intermediary about progress: the case where candidates were to contact me, but I heard nothing, would be included in the category 'refusal evidenced by candidate's failure to contact'; or if the candidate contacted me, or I contacted him, and he did not want to partake this became a 'direct refusal'. In the first case, the whole counting process is predicated upon trust: the researcher trusts that the intermediary has indeed contacted the candidate. In one case a candidate was identified via a previous male who had been interviewed, and he (Brian) subsequently agreed to be interviewed. After the interview Brian also said he knew another man that he could ask on my behalf. I was very grateful both for his agreement to be interviewed and his offer to contact another candidate. Several weeks elapsed with no

word from Brian, so I telephoned and asked him what had happened. He replied that he had contacted the candidate, but was told that there was no interest in being interviewed. I left it at that and thanked him for his efforts. However, I was sure that he was lying: I just knew, without being able to specify how, that he had not contacted that person, that he had made up the story about the candidate's refusal.

In the practical process of tallying non-response what is to be done with occurrences such as this? Should it be added to the category 'direct refusal by candidate' or would that be a mistake given the strong hunch that the candidate was in fact never asked to respond? What category would it then go into? Final rates of non-response presented numerically, apparently transcend these practical decisions, but obviously this whole quantitative process is based upon 'reasonable' interpretations within the context of pragmatic problems that researchers face. In reality, research is an often messy process, and the point of the above example is **not** that the whole process of producing figures of response and non-response is biased, or error prone, but that it is socially accomplished, and this accomplishment is based upon common-sense, everyday, rationalisations and procedures.

This point can now be pursued further by presenting the data corpus and analysing the accounts of non-response it contains. The analysis is divided into four parts. First, the data corpus is presented along with some initial comments; second, the accounts of non-response in these data are described; third, attention is turned to a central task that accounts of non-response achieve - the attribution of motives; and fourth, the above points are more narrowly focused on one example

of accounting work which is more directly concerned with the fertility-virility linkage.

5.4 *Data and Some Initial Comments*

The data corpus comprises eight extracts from social scientific studies of infertility which include comment on non-response:

1. Humphrey (1969: 8)

This response rate, amounting to 86 per cent of the couples presumed to have been reached, was regarded as encouraging. As in all such enquiries, the non-respondents may have differed in important respects: for example, some may have been unduly sensitive about their childlessness, others lacking in a social conscience ... Even those who could not be reached may have had more than their fair share of unresolved problems, or perhaps in some cases the marriage had been dissolved. All this is sheer speculation, but one suggestive finding emerged from the clinical summaries for these groups. This was that the sixty-eight couples whose questionnaire was returned undelivered, and the thirty-six who apparently received one but did not return it, showed rather more impairment in their fertility tests than the 216 respondents.

2. Miall (1986: 271)

I began my research with the aim of comparing reactions of men and women to their involuntary childlessness. However, I was unable to obtain a sample of men sufficient for this comparative approach. Indeed, in order to recruit female respondents, I was obliged to use a snowball sampling technique. The difficulty that I had in obtaining a sample of infertile individuals who would participate in my study reflects the sensitivity and secrecy that surround the problem of infertility.

3. Miall (1989: 298, fn. 3)

Male infertility may be more stigmatizing than female infertility, given the link between male potency and fertility (cf. Miall, 1986). Men may have been more reluctant to discuss personal infertility. In addition, our culture continues to link women more explicitly to reproduction and family than men. Thus, men may have had less interest in participating in research on these issues.

4. Sabatelli, Meth, and Gavazzi (1988: 339, 342)

... data were collected from 62 women and 36 men (a 64% response rate) ... Unfortunately, because of missing data or because of a recent birth ... the final sample used within this study consists of 52 women and 29 men

...
In addition, males and, in particular, males with an infertility condition, are underrepresented in the sample. This underrepresentation may be due to the unwillingness of males to participate in research on sensitive issues

5. Becker and Nachtigall (1991: 877)

A study of 43 American couples examined women's and men's experience of infertility treatment Data were collected with 28 couples during a one-and-a-half year period in 1984-85. An additional 8 couples and 7 women who chose to be interviewed without their partner were interviewed in a second wave of data collection ... The 7 women interviewed alone were willing to participate only if their partners were not included. They gave various reasons, such as, "He's too busy," "It would add stress to the relationship," and "I don't want to bother him." There were no refusals to participate in the research.

6. Woollett (1985: 475)

Fifty infertile people were interviewed.... [consisting of] Forty-two women (mean age 31.4 years, range 24-41) and eight men (mean age 36.9 years, range 29-50) ... Women were more

willing to discuss infertility than men, reflecting assumptions that children are less important to men (Owen 1982).

7. MacNab (1984: 91-93)

The findings of the study are contained in microcosm in this description of the difficulties of finding men to participate in the study. The medical clinics explained that their average male infertility patient did not return for a second appointment, often avoiding even the simplest evaluation procedures. None of the men in the study are from this referral source. The urologist stated that the men whom he dealt with were usually so devastated that they could not talk about what the experience meant to them. ... Even men who were friends of the researcher's friends and colleagues did not consistently return the questionnaires. These men had generally had direct phone contact with the researcher, and had agreed to participate in the study. Many of them did not respond to requests that they state their reasons for withdrawing from the study. Two men explained that their wives felt uncomfortable about their sharing such personal information with an outsider. One man wrote that he was too busy to fill out the questionnaire. Most others did not give reasons for changing their minds about participating. ... What seems to make the critical difference in their willingness to join the study is the direct personal connection they feel between the referral source and themselves. ... This is evidence that the topic is emotionally loaded. ... The reasons for the silence that has prevailed about men and infertility are manifest in these difficulties. The need for a forum for men struggling with the problem is both evident and denied.

8. Link and Darling (1986: 49, 57)

Fifty physicians were randomly selected from the membership list of the American Fertility Society and contacted to enlist their cooperation with this study. Since only six physicians agreed to participate, additional respondents were obtained ... The difficulty in obtaining respondents resulted from the reluctance of physicians to participate in research involving their patients, as well as problems which couples experience in discussing their infertility with persons other than physicians or counsellors. The resultant sample consisted of 43 couples ...

Furthermore, the researchers were provided with data from an additional 17 wives whose husbands chose not to participate in the study.

...
Since the data were anonymous, the reasons why the husbands chose not to participate are unknown.

Initially, two questions can be asked about the data: why are these particular extracts offered as data?; and how comparable are the extracts? After answering these questions a brief summary will be made of the data and then analysis proper can begin.

1. Why are these particular extracts offered as data?

Obviously, these extracts do not constitute all of the comment on non-response within the infertility literature, so the question 'why these extracts' is germane. It is not easy to find extended discussion of the researchers' problem of non-response, and a first answer to the question is that these extracts constitute some of the more extensive comment on this issue. But, more importantly, they are chosen as data because as well as containing information on non-response, they offer an explanation of it, and this is the more crucial component of interest. Further, with the exception of extract 7, all the extracts are from published studies, and are good examples of the type of research conducted on infertility. Overall, the extracts provide a good indication of the variety and patterned nature of accounting work on non-response.

2. How comparable are the extracts?

Broadly speaking, the data above can be conceptualised as the result of a research process involving three elements: i), a population which,

ii), is sampled by researchers to gain information about the population, where, iii), the sampling may be conducted in different ways. If one was concerned with the comparability of different studies of infertility, problems could arise firstly in the definition of the population - we would want to be sure that we were talking about the same thing when comparing studies of infertility. However, such a uniform base does not always occur in the infertility literature, because there is a serious lack of clarity in the use of the term 'infertility'. While a couple may be described as 'infertile', and in this sense both the man and woman experience infertility, it is often the case that only one partner in the couple will be identified as 'reproductively impaired'. As one astute commentator has observed, "For women, the single most common cause of infertility is ... mating with an infertile man. (Conversely, the single most common cause of infertility in men is mating with an infertile woman.)" (Rothman, 1982, as quoted in Greil, Leitko, & Porter, 1988: 175). Obviously, infertility can also involve the situation where both the man and woman in the couple are reproductively impaired, or, where the infertility is due to unexplained factors. Lack of clarity arises because infertility researchers do not always make it clear what situation they are referring to when they use the terms 'male infertility' and/or 'female infertility'. Further, there may be insufficient information within the published study to clarify matters. This results in the situation where it is far from clear what the referents of the terms 'infertility', 'female infertility' and 'male infertility' are, hence comparability across studies is problematic.⁶

⁶ Recall here the discussion in chapter one about bracketing 'male infertility'. This point provides another good reason for the use of brackets.

In addition, the studies are not fully comparable because they do use differing sampling methods, and it is reasonable to assume that this is related to the variation in response rates and perhaps even to the gender imbalance in respondents.

Having noted this problem of comparability, essentially it will be sidestepped in the following analysis. In practical terms, it is not possible to standardise the terms used in the extracts that form the data corpus. But more importantly, the essence of male or female infertility is not the topic of interest here. The interest is in how common-sense figures in social science accounts of non-response, and a good argument can be made that irrespective of whether the accounts lack a standard referent, common-sense will figure in the accounts as a background, unexplicated feature.

3. Summary of the data.

Even though the number of extracts is small, the number is sufficient to see variability and overall patterns in the data corpus, as Table 5.2 indicates.

Table 5.2

Summary of Extracts on Non-Response

Extract	Response Rate	Ratio of female respondents to male respondents
1	86%	2.5 : 1 (80/32) ^a
2,3	not given	confined to women
4	64%	1.8 : 1 (52/29)
5	100% (92%) ^b	1.2 : 1 (43/36)
6	not given	5.3 : 1 (42/8)
7	5% ^c	confined to men
8	not given	1.4 : 1 (60/43)

- a This is the sub-sample that Humphrey interviewed, the 86% response rate is for the initial sample from which those interviewed were drawn.
- b Response can be calculated as 92% if one counts the 7 women who chose to be interviewed without their partner as 7 de facto cases of male non-response.
- c MacNab does not give a non-response rate; this is calculated from his outline of questionnaires sent out (N=approx. 600) and final number of respondents (N=30).

The data's variability is clearly shown in the range of response rates: from Link and Darling's 100% (or 92%) (extract 5), to MacNab's 5% (extract 7). While it can be ascertained from the comments of those researchers who did not give a response rate, that the non-response rate was high, the range from 5% to 100% is still very large. In general this would suggest that studies of infertility will not invariably

face poor response rates (and obviously the differing sampling methods need to be borne in mind here). Conversely, the table also displays a clear uniformity - the gender imbalance in the samples. For every case where a ratio can be calculated, females outnumber males, with the ratios ranging from 1.2 : 1, to 5.3 : 1. Thus, in these studies men have displayed a greater reluctance than women to participate.

5.5 *Accounts of Non-response*

Before proceeding to describe the accounts of non-response, it is worth outlining what is meant here by the term 'account'. Within the wide sociological literature centrally concerned with accounts (eg. see Antaki, 1988; Gilbert & Abell, 1983; Scott & Lyman, 1968) there is some variation in definition, but Heritage's discussion of an account as an "overt explanation in which social actors give accounts of what they are doing in terms of reasons, motives or causes" (1988: 128) is a generally acceptable definition. In simple terms, an account is not just a description of an event but is an explanation of how or why it happened. Such explanatory work may take many forms, from explicit delineation of causal linkages, to taken-for-granted inferences about motives for action. Again, most commentators would recognise this variability in accounts, but further complicating the concept of account is the notion of 'accountability', which is a central precept of ethnomethodological inquiry. While accountability may involve accounts, the process it conceptualises is more than the simple use of

accounts as explanations. As Garfinkel put it in the first page of *Studies in Ethnomethodology*, "... the activities whereby members produce and manage settings of organized everyday affairs are identical with members' procedures for making those settings "account-able" (1967: 1). A verbalised account is one very common and important way that activities are rendered "account-able", but gazes, body movement, gestures, and so on, also contribute to the accountability of activities. Overall, the concept of accountability emphasizes the situated nature of accounts: accounts "organize, and are organized by, the empirical circumstances in which they occur" (Heritage, 1984: 141), and together, accounts and accountability constitute a 'running index' of implicit understandings continuously updated in the course of practical activities (Heritage, 1988).

While textual accounts, such as the extracts above, can be examined with respect to accountability (see McHoul, 1982), this approach will not be pursued here. Rather, the focus will be on the accounts as explanations. Clearly the data corpus represents social scientific accounting work on the problem of non-response. In essence, the account consists of an explanation of non-response, where this involves attributions of reasons, causes and motives. This attribution work is a prime area where important linkages between social science and common-sense occur, and this will be discussed in more detail after the three main types of account are described, as follows.

1. Sensitivity

This appears to be the dominant account in the extracts. Very simply the explanation is that non-response is an indication of the

sensitivity of infertility - it is a delicate matter and hence people may not wish to talk to a researcher (or others) about it. Forms of this account can be seen in extracts 1, 2, 4, and 7, where, respectively, the following phrases are linked to infertility as the underlying reason for non-response: "sensitivity", "sensitivity and secrecy", "sensitive issues" and "emotionally loaded". The simplest invocation of a sensitivity account is seen in extract 4, where Sabatelli, Meth and Gavazzi state, "This underrepresentation [of males] may be due to the unwillingness of males to participate in research on sensitive issues".

In its general form, 'sensitivity' may be used to account for non-response by both men and women. For example, Miall (extract 1) wanted to interview both men and women but clearly had a problem with male non-response, which she explains via a sensitivity account (extract 3); but, in addition, she suggests that the fact that she had to resort to snowball sampling shows that gaining female respondents was also a problem (extract 2). Thus, Miall offers the "sensitivity and secrecy" of infertility as an account of non-response, for both genders. Although not so explicitly, Link and Darling (extract 8) suggest that their problem in gaining a sample of infertile couples, as well as being due to the reluctance of physicians to cooperate, was due to "problems which couples experience in discussing their infertility". This seems to fit within a sensitivity account, and again is applicable to both men and women.

In addition, there is a more specific form of the sensitivity account specifically offered for male non-response. This is the fertility-virility linkage, which might be considered as a subset of the

sensitivity account applying specifically to men. The clearest statement of it comes from Miall: "Male infertility may be more stigmatising than female infertility, given the link between male potency and fertility (cf. Miall, 1986). Men may have been more reluctant to discuss personal infertility ..." (extract 2). There are no other clear examples of the fertility-virility linkage account in the extracts but a further example will be discussed below in section 5.7. It should also be pointed out that researchers often make comments about the fertility-virility linkage in discussion not concerned with non-response. For example, MacNab, identifies the links between masculinity and fertility as an important part of the experience of male infertility, but, he does not take this up as an explicit account of the real difficulty he had in gaining respondents.

2. Children are not so important for men

As outlined in the introduction, consideration of the question of gender differences in response to infertility has produced a consensus that women are more adversely affected by infertility than men. A very common explanation for this involves, to put it simply, socialisation into normative roles. It is argued that in our culture the socialisation process places high value on women as mothers, hence this becomes a natural and normal role for women. A correlate of this explanation is that the role of father and the work of childrearing is not so important for men (see Berg, Wilson & Weingartner, 1991; Humphrey, 1977; Nock, 1987). This correlate then becomes available for accounting work on non-response. Extracts 3 and 6 are two clear examples of this account: "In addition, our culture continues to link women more explicitly to reproduction and family than men. Thus,

men may have had less interest in participating in research on these issues" (3); "Women were more willing to discuss infertility than men, reflecting assumptions that children are less important to men (Owen 1982)" (6). While not as common as the sensitivity account, this account is offered, and has attractive linkages to other research on gender roles and childrearing.

3. Pragmatic and Other factors

There are three extracts (1, 5, 7) which include this account. In two out of the three examples, the account is made via straight quotes from, or reports of, the subjects' discourse, rather than as researchers' direct explanations: "They gave various reasons, such as, "He's too busy," "It would add stress to the relationship," and "I don't want to bother him." (5); "Two men explained that their wives felt uncomfortable about their sharing such personal information with an outsider. One man wrote that he was too busy to fill out the questionnaire" (7). While some of these reports contain matters that suggest links with a sensitivity account, there is a common emphasis on pragmatism over and above the possibility of sensitivity: being too busy; respecting their partner's wishes; not wanting to add stress to a relationship.

One account that clearly belongs in an 'other' category is contained in extract 1. Humphrey makes the interesting comment that, "As in all such enquiries, the non-respondents may have differed in important respects: for example, some may have been unduly sensitive about their childlessness, others lacking in a social conscience" (extract 1). While Humphrey offers the dominant

sensitivity account, he also makes the suggestion that non-respondents were "lacking in a social conscience" - this is the sole occurrence of such an account.⁷

These then, are the three main accounts which can be identified in the data corpus. Each extract may contain two or more of the accounts, and there appears to be no sense in which they are mutually incompatible. Clearly, the 'sensitivity' account is the most common, with the other two being offered as additional explanations.

5.6 *A Central Task in Accounts of Non-Response: The Attribution of Motives*

Clearly, in the accounts of non-response the issue to be explained is precisely why the candidates did not wish to partake in the researchers' studies. Thus, for the sake of brevity, part of the work

⁷ Humphrey appears to be assuming that it is morally correct for subjects to respond to researchers' requests for information, and that by agreeing to participate, respondents contribute to the social good. If this is the assumption he worked upon when he conducted his study, in the light of recent discussions of the role of social science in society (eg. Foucault, 1980) it now seems quaint and ill-considered. However, it cannot be dismissed that easily. It is still an extremely common social science research practice to couch requests for information from the 'natives' in terms of the possible societal benefits to be accrued from the research. Whether researchers themselves really believe this may be immaterial, for this whole way of thinking about social science seems to have embedded itself in everyday understandings of social science to the extent that the 'natives' themselves expect requests to be couched in these terms. Time and time again, when I spoke to people about my research and when I made various requests for respondents, the people I spoke to would assume I was doing a 'survey' of male infertility in order to find out about its frequency and what needed to be done about it. While research obviously does proceed in this manner, just as obviously it can take other forms. Nevertheless, it is the 'survey' view of research and the assumption that policy changes will be offered, that seems pervasive in everyday discourse about sociology.

that the accounts accomplish can be labelled 'attribution of motives'. The question pursued in this section is based upon the assumption that the attributions of motive contained in the accounts are perfectly plausible - they do not seem strained but are reasonable explanations. But of course, the question is, how is this plausibility accomplished, how is the naturalness of the accounts produced? In the extracts the attributions of motive are made, mostly, as blunt assertions - they appear to be so obvious as to not need explaining. But given the emphasis within social science on reasoned argument and the provision of evidence for generalisation, it would be surprising if blunt assertions were all that were offered. Indeed, five forms of motive attribution work can be identified in the accounts, and these are discussed below.

1. 'No comment, but ...'

In many research situations there is little possibility of non-respondents giving any information on their motives for non-response. Hence, one possible solution to the question of motives is to accept this lack of information and avoid comment altogether. Extract 8 appears to be an example of this. Link and Darling's initial sample of 43 couples was supplemented by "17 wives whose husbands chose not to participate in the study". While Link and Darling used the wives' responses as data, they restricted comment on the husbands' non-response to the sole statement that "Since the data were anonymous, the reasons why the husbands chose not to participate are unknown". In effect, they make no comment. While this seems to be a case of adherence to a relatively strict scientific

model of social science, where if there is no evidence there is no comment offered, it should be noted that Link and Darling previously invoked the sensitivity account in relation to their problem in gaining couple respondents. Thus, this is not an absolute case of 'no comment': 'no comment' leaves the motive attribution work up to the reader, but this work may well be informed by Link and Darling's previous 'sensitivity account', which provides a minimal frame for understanding the subsequent mention of the husbands' non-response.⁸

2. 'The candidates said ...'

In some research situations non-respondents (and others who speak for them) do provide at least minimal information on why they did not respond. In this situation, a relatively simple option for providing the grounds of motive attribution is simple reportage of what information non-respondents offer. Extracts 5 and 7 include examples of this:

Two men explained that their wives felt uncomfortable about their sharing such personal information with an outsider. One man wrote that he was too busy to fill out the questionnaire. (7)

They gave various reasons, such as, "He's too busy," "It would add stress to the relationship," and "I don't want to bother him." (5)

The extracts differ in that Becker and Nachtigall (5) directly quote three reasons for non-response by husbands, while MacNab (7) does

⁸ In general, an absolute case of 'no comment' on high non-response seems very unlikely. Given the 'observable-reportable' nature of absence of a second pair part, some comment appears to be a socially sanctionable matter - within both the world of everyday life and social science. Certainly in the case of high non-response rates, absence of comment would itself draw comment - it naturally calls for accounting work.

not opt for direct quotation. But, both show the general technique of reporting what the candidates said.

Some research from conversation analysis suggests an interesting point about the use of such a technique. Pomerantz's research (1984) on 'the practice in conversation of telling 'how I know"' seems particularly germane. As part of her paper she discusses excuse making, and argues that in the situation where someone repeats an excuse they must take some position with respect to the validity of the excuse. Simple repetition of the excuse takes the position that the excuse is valid and accurate, whereas reportage of only the circumstances of hearing the excuse does not imply acceptance of the excuse, but displays scepticism and caution about the acceptability of the excuse. Now, in the use of 'the candidates said ...' technique, simple repetition of motives for non-response appears. While giving a motive for non-response is not exactly the same as giving an excuse, it is similar; yet, my feeling is that the simple repetition does not accomplish what it does in ordinary conversation. That is, the overall tone of the extracts which use 'the candidates said ...' technique is one of scepticism and caution, and not acceptance of the proffered motive. Reporting that a candidate said "he was too busy to fill out the questionnaire" invites scepticism - in short, it seems like a convenient lie.

This inversion of an everyday technique and what it accomplishes, seems explicable by reference to the operation of an underlying assumption. This point will be taken up more extensively in the conclusion, but simply, the suggestion is that the inversion occurs

because prior to the offers of what the candidates said, there has been massive adherence to the notion that infertility is a sensitive matter. To first discuss the sensitivity of infertility, and then to offer "being too busy" as an 'excuse' for non-response, inverts the effect of simple repetition as a warrant for excuse-acceptance. While speculative, the feeling when reading the pragmatic accounts is that the reasons for non-response are to be treated with caution. This represents an interesting intertwining of common-sense and social science.

3. 'Other evidence suggests ...'

The clearest example here is in the extract (1) from Humphrey's work. Because his initial sample was gained through an infertility clinic, Humphrey was able to compare non-respondents' and respondents' medical records. Thus, while his initial comments on the reasons for non-response are, as admitted, "sheer speculation", access to the medical records enables the addition of a more 'objective' scientific procedure - the use of cross validating information - into his accounting and motive attribution work. The result is, as he puts it, the "suggestive finding" that non-respondents "showed rather more impairment in their fertility tests than the 216 respondents". He does not discuss this any further (except in a footnote which states that the difference was not statistically significant), but it is notable that he is clearly leaving the presumed linkage between greater impairment and non-response unstated and implicit. The sense of his argument is dependent upon readers' abilities to fill in the missing clause, to make the complete statement that, the worse the fertility impairment, the greater the social sensitivity, and hence a greater likelihood of non-response. More generally this could be seen as an example of an

everyday naturalised way of thinking about illness and disease: the worse the degree of one's illness or impairment the greater sensitivity one should feel about it. In this case then, the common scientific procedure of using cross validating evidence has common-sense assumptions about reactions to illness heavily embedded within it.

4. 'Research shows ...'

There are two examples of reference to previous research in the extracts. Miall (extract 3) uses a citation to her own work:

Male infertility may be more stigmatising than female infertility, given the link between male potency and fertility (cf. Miall, 1986). Men may have been more reluctant to discuss personal infertility.

and Woollett (extract 6) refers to other research:

Women were more willing to discuss infertility than men, reflecting assumptions that children are less important to men (Owen 1982).

Clearly, references function as a persuasive device, suggesting that there is evidence to bolster the claims being made (Gilbert, 1977). In the first case it is the fertility-virility linkage version of the 'sensitivity' account that is strengthened by the reference, while in the second it is the 'children are not so important for men' account that is strengthened. But equally as clearly, there is a great deal of glossing occurring here: a whole research paper is summarised as positive evidence for the substance of the accounts. Obviously, academic readers who are familiar with the literature being cited can make up their own minds about whether the citation displays the knowledge as glossed; and, of course, given that academics do engage in critique of

other academic work, disagreement is always a possibility. But, the interesting point here is that one cannot doubt everything all of the time - the simplest option when reading social science discourse is to accept that references to other research do indeed provide evidence to fit the argument. Just as in everyday life where people accept what other people say most of the time, the very mundanity of referencing contributes to its effectiveness as a persuasive device, and more generally contributes to the continual re-establishment of consensus in particular research areas (Gilbert, 1977).

Moreover, in this process there is the potential for weak research findings to become reified as strong and accurate findings. This possibility can be seen in Miall's referencing work. She cites her own work in relation to the fertility-virility linkage, but this work actually contains no direct comment from men on this issue - it is based upon comments from their partners. It is not being suggested here that evidence on the fertility-virility linkage must come from men, rather, it is being emphasised that interview talk between a researcher and a female partner about how the male partner experienced infertility will have its own organisational exigencies which cannot but be implicated in the content of such talk. In the 1986 research that Miall cites, the question of the socially organised manner in which women might speak for their male partners is not considered: there is an assumption that asking about another's experience as an interaction itself, is unrelated to the content of what is being reported, that is, their partner's experience of infertility.⁹ Despite these problems it is

⁹ This is a standard ethnomethodological argument reproduced in any number of works. For an empirical analysis of the interactional accomplishment of representing others' experience see Perakyla and Silverman (1991a). And for a

clear that the aim of citation is to give her accounting work greater persuasive force.

5. 'Everyone knows ...'

While the above devices are used sporadically in the extracts, a great deal of the motive attribution work contained in the accounts is achieved by simple declarative statements of the form, A is well known about infertility, and this clearly explains B (non-response). Consider Miall's statement that

The difficulty that I had in obtaining a sample of infertile individuals who would participate in my study reflects the sensitivity and secrecy that surround the problem of infertility.
(2)

This displays a simple declarative linkage between infertility and sensitivity, which constitutes the explanation for non-response. The linkage between infertility and sensitivity is offered here as something that 'Everyone knows' - no references are supplied, no quotations offered, it is simply assumed that the facts of the matter are now well established (extracts 1, 4, 7, and 8 also contain similar examples). Also, there is a second order of invocation of what 'Everyone knows' occurring here. Miall does not need to say exactly how the sensitivity and secrecy of infertility led to her problems with non-response: it is just assumed that we know and can fill in the other logical clause - people with sensitive conditions will often avoid talking about them. That is, the attributed motive for non-response is simply one of 'pain avoidance' - people will avoid talking about things that have pained them.

more theoretical discussion germane to wives speaking for husbands (and vice versa) see Gubrium and Holstein (1987).

Hence, Miall is relying on the fact that in everyday life "persons' actions are all too readily explicable" (Sharrock & Button, 1991: 165). This 'ready explicability' has a great deal to do with the accountability of action: persons' actions occur within social scenes and motives for action are displayed and recognised incarnately within those scenes. That is, they are a dynamic and embodied feature of the very action that they can be thought to explain. Further, the incarnate or reflexive nature of accounts and motive attribution is built up over time into publicly available modes of understanding human action - there are a wide stock of motive attributions which 'go without saying' as explanations of action.¹⁰ The point to be taken here is that social scientists are equally reliant on publicly available modes of understanding in their accounting work on infertility and non-response.

To recall the earlier discussion, we could say that just as the sociologist of education has no trouble in finding 'classrooms', and 'teachers' and 'pupils' within those classrooms, the infertility researcher has no trouble finding infertility as a 'problem', as a 'sensitive' matter, and inferring motives for non-response from this underlying typification. As Watson notes, "Motives are so inextricably built into the description of deeds that the imputed

¹⁰ While some sociologists would prefer the 'essentialist' tack of positing motives as the direct property of individuals (and hence cognition) there is a strong argument that motives can be more profitably dealt with as objects of available and public modes of understanding. Mills (1940) is the seminal work here, but later work has been more inspired by ordinary language philosophy and ethnomethodology (eg. see Coulter, 1979; Watson, 1983). For an illustrative debate between a traditional sociological approach and ethnomethodological approach to motives, see Bruce and Wallis (1983, 1985) and Sharrock and Watson (1984, 1986).

motive makes the deed publicly visible "for what it is" (1983: 42). Moreover, there are strong connections between imputation of motive and society as a moral order:

The imputation of motive(s) is part and parcel of the many ways in which society members ... articulate and apply the logic of the moral order. To impute motives is one way in which we not only make sense, but *moral* sense of conduct.
(Watson, 1983: 43, original emphasis)

Hence, the prevalence of 'Everyone knows' forms of motive attribution in the accounts of non-response: these explanations are able to be left at a taken-for-granted level because they are morally sanctionable descriptions. In effect, to doubt the efficacy of such a motive attribution would be to risk negative moral implications. Despite social science operating with a notion of scepticism, it is heavily dependent on motive attributions that have a taken-for-granted, and hence, moral force.

5.7 *One Further Account of Non-response Examined for an Important Implication of Accounting Work*

As noted in section 5.2, the point of this discussion has not been to criticise social scientists for using common-sense in their accounting work. Nor, on the other hand, has there been any occasion to argue that common-sense is faulty when compared to social science. Despite the absence of this latter argument here, it is not uncommon within social science, as Garfinkel pointed out some time ago in the Purdue Symposium:

For example, the sociologists are forever beginning their descriptive works, "whereas it is commonly held that ..." then comes the *corrective*. The case is otherwise.

All of this means that the common-sense knowledge of the member is put up against something that we call scientific knowledge, where scientific knowledge is a *competitor* to common-sense knowledge (Garfinkel in Hill and Crittenden 1968:15)

(as quoted in Flynn, 1991: 219)

Obviously, in the case of negative stereotypes and 'myths' there are good grounds for the sociologists' debunking activities, but Garfinkel is referring here to a much more general operation that commonly occurs in the social sciences. In short, this is the operation of irony. This, and the above-mentioned moral power of public modes of understanding, can be clearly seen in Owens' account of non-response and his related invocation of the fertility-virility linkage, which is discussed below.

Owens (1982, 1986) studied involuntary childlessness in Wales, focusing on reasons couples have for wanting children and the possible consequences of childlessness, giving close attention to both men and women within the infertile couples. The two main parts of his research consisted of interviews with 30 working class couples attending an infertility clinic, and a questionnaire study of the National Association for the Childless (NAC). In both these areas Owens found men more reluctant to partake in his study than women. In the infertility clinic study, 44 couples were eligible but 14 refused to partake, of which seven were cases where the wife agreed to partake but the husband refused. The NAC study showed an even

greater gender disparity: 120 questionnaires were sent out with 80 per cent of wives replying, but only 20 per cent of husbands doing so.

Owens provides several possible explanatory factors for the greater rate of male non-response: men may be less interested in reproductive matters than their wives; men may see women as more responsible for these matters and this may extend to the completion of questionnaires; there may be structural factors such as men being in employment making them less able to come to infertility clinics; and it may also be that medical clinics are oriented to women. Many of these factors are not present in the accounts in the data corpus, and in this sense Owens provides the most comprehensive discussion of the possible explanatory factors for non-response.

However, he does emphasise that the list is suggestive, and further research would be needed to confirm matters. Amongst all this, Owens concludes by saying that

The most intuitively plausible hypothesis however concerns male sensitivities on these matters. Some of the literature cited in Chapter II has alluded to this, and subsequent parts of the thesis reinforce these views. There are good grounds for believing that men are more reluctant to participate in studies such as these, because they are more threatened by them. (1986: 4.45-4.46)

Thus, faced with the problem of lack of evidence on motives for non-response, Owens opts to invoke a sensitivity account as the "most intuitively plausible". The very words "intuitively plausible" also indicate Owens' first device for attributing motive, that is, "intuitively plausible" equates with what 'Everyone knows'. The quote also displays the beginnings of another device noted above - a literature is

referred to ('Research shows ...') - and both of these devices are used in the "subsequent parts of the thesis" where Owens provides more grounds for the invocation of the sensitivity account. This turns out to revolve around the fertility-virility linkage, as follows.¹¹

The first part of Owens more developed argument is to refer to the "considerable evidence that the diagnosis of infertility has a greater impact on the man than on the woman" (1982: 80) due to the fertility-virility linkage. In both works, Owens cites Burnage (1977) and Humphrey (1969) as part of this "considerable evidence".¹² But, on examination, the Burnage reference turns out to be a research-in-progress report of barely two pages. Burnage gives her research method as: "I have been talking to approximately 100 infertile and subfertile patients in two London teaching hospitals about the effect infertility has had, and is having, on their lives" (Burnage, 1977: 47). It is not stated whether infertile men were or were not spoken to. Overall, Burnage's paper is an excellent example of "anecdotalism" in sociology (see Silverman, 1985), and the useful anecdote that Owens finds amongst it, and quotes, is that "The discovery that he is infertile can be an enormous blow to a man's ego and, combined with his wife's keen desire to have children, can leave him feeling extremely inadequate" (1977: 48). This may well be so, and equally, it may well not be so: from what the paper contains there is no way of deciding. The point here is not to be cynical about Burnage's research,

¹¹ Note that discussion covers both Owens' 1982 and 1986 work - they present virtually identical arguments.

¹² In fact, in the 1982 paper Burnage and Humphrey are the only research works Owens cites, while in the 1986 thesis he makes another reference to Rainwater (1960), but does not discuss this work.

but to emphasise that regardless of its substance, in its incorporation in social scientific discourse via the method of referencing it accomplishes plausibility for Owens' argument.

Owens also cites Humphrey on the fertility-virility linkage and a similar process can be seen. Here is Humphrey's argument from which Owens quotes:

The view expressed by Schellen (1957), that awareness of sterility inflicts greater trauma on a man than a woman in our culture, is **widely accepted**. Whilst a sense of failure may be common to both sexes, it is only the proud male who regards it as an affront to his sexual capacity. For him procreation has always served as a means of demonstrating his virility, whereas **it is well known** that a woman's fertility gives no indication of her sexual responsiveness. And no matter how bravely he has accepted the discovery at a conscious level, unconsciously the equation with impotence is likely to remain. (Humphrey, 1969: 52, emphasis added)

Owens quotes this in both his paper and thesis, and it is worth reproducing the quote here because it neatly shows the power of the referencing process. As can be seen Humphrey himself combines an initial reference to a social scientific work - Schellen - with arguments about what 'Everyone knows': the truth of the fertility-virility linkage being "widely accepted", whereas in contrast its absence in women is "well known".¹³ Clearly, Owens quotes Humphrey to bolster his

¹³ Humphrey's invocation of the well know fact that a "women's fertility gives no indication of her sexual responsiveness" is interesting in the light of historical research. Lacquer (1990), for example, shows how in past centuries it was thought that for conception to occur a woman must have an orgasm. Thus, if Humphrey is to be believed, the commonsense assumption of no connection between a woman's fertility and her sexual responsiveness may be a relatively recent understanding. Also note that Humphrey, in his desire to argue the connection between fertility and virility, sets up a no-lose situation via invocation of the unconscious: "unconsciously the equation with impotence is likely to remain". That is, even if men openly deny any threat to their virility, the threat is still there at some essential, hidden, unconscious level.

sensitivity account of non-response, and, in effect, this makes Owens' referencing work a third generation comment about the fertility-virility linkage: Schellen notes the link between male fertility and virility, Humphrey quotes Schellen, and Humphrey in turn is quoted by Owens. Of course, the simple point to be noted here is that common-sense declarations are deeply embedded throughout this process - see the highlighted phrases for example. No doubt if Schellen were consulted a similar dependence on common-sense could be found.

The upshot is that the intertwining of common-sense and social science has a long lineage with respect to the fertility-virility linkage. It is not as if resort to common-sense is hidden in this lineage, but, on the other hand, given that this is social science discourse it is not made explicit either: it might be considered "seen but unnoticed", to use Garfinkel's (1967) felicitous phrase. The end result is that Owens can confidently state that there is "considerable evidence" for the fertility-virility linkage.

Having made this first step, Owens then considers his data in the light of existent social science knowledge on the fertility-virility linkage. And here the whole process takes on some interesting twists: we see a classic example of what Pollner (1987), after Laing, has called the "politics of experience". This happens in the following way. Owens' knowledge of the social science literature and his common-sense tell him that when studying male infertility it is important to look for signs of infertility as a 'threat to male virility'. He then talks to men within infertile couples, but concludes that

"Such threats to virility were not, however, easily discerned. Indeed the vast majority of respondents denied them" (1982, 81). Or, as put in the later work, "most of the men did not acknowledge such threats, some denying them outright, others saying that the diagnosis of infertility could be taken as a matter of fact ..." (1986: 8.13). This presents Owens with what Pollner calls a "reality disjuncture" (1987: 69), that is, a contradictory experience of the world, a puzzling event: Owens cannot find in the men's accounts what social science research tells him should be there.

Pollner's research into mundane reason suggests that when faced with such a disjuncture, members resolve it by "formulating one or another (and perhaps both) of the competing versions of reality as the product of an exceptional method of observation, experience or reportage" (Pollner, 1987, 69). Owens resorts to exactly such a method to resolve his particular disjuncture, and this resolution has two parts. First, in his 1982 paper he states: "Nevertheless, given the strength of Humphrey's arguments above, it would be naive to accept these statements simply at face value" (1982, 81). He then introduces an appeal to cross-validation by arguing that more evidence on the threat to the male ego came from the men's wives.

This resort to the wife's reports, is the second strand which is elaborated in Owens' thesis:

However, despite the husbands' denial of the threats to their virility, there was good reason not to accept them at face value. Of course it is possible that the men did not feel that their virility was threatened because the concept held no salience for them, or because they measured it according to other

unspecified criteria. But evidence that the men may well have felt insecure came from the wives and a number commented on what they felt to be their husband's sensitivities on these matters. (1986: 8.14)

Owens then offers some extracts from the wives' comments, and goes on to suggest that the notion of a fertility-virility linkage need not be abandoned, only altered. He suggests that "whilst the respondents were not wholly forthcoming about any blow to their self-concepts as men, they did speak of the threat that infertility would raise to the fulfilment of their role as husbands" (1986: 8.15, original emphasis). It is in this "complementary hypothesis", as he calls it, that Owens seeks to maintain the robustness of both the sensitivity account and the fertility-virility linkage.

Either way, the sum effect is that the men's reportage is presented as exceptional: it hides the truth; it cannot be accepted at "face value"; it does not correspond with the reality that social science knowledge, and common-sense modes of understanding, say should be there. Hence, the resolution of the reality disjuncture centres on acceptance of a social scientist's supposedly strong prior argument, or on the wife's reports of how their husbands experienced infertility. One consequence of choosing these views over the infertile men's accounts, is that their ability to report their own experience is discredited. Hence, we have a "politics of experience".

The question has to be asked whether social scientists should be engaging in such practices. In my view, the short answer is no. This is not to suggest that infertile men have a privileged viewpoint of infertility, nor that we should find some better method of capturing

the experience of male infertility. Rather, it is more a critical correlate of the argument that if one is concerned with studying the impact of infertility on either gender, then a profitable way to proceed is by considering the procedural mechanisms through which social phenomena are made available. Invoking common-sense is one such procedure.

5.8 *Concluding Discussion*

There is nothing new in the argument that social scientists depend upon common-sense and unexplicated background knowledge in their research. However, just how important common-sense is to social science, and just how intricate the linkages between common-sense and social science are, cannot be realised without detailed investigation of social science practices. This was the task of this chapter, and while the analysis has remained at a relatively simple level, the pervasiveness of common-sense reasoning in social science, and the complexity of the linkage between the two, has certainly been shown through examination of accounts of non-response.

To recap, within the infertility literature researchers have noted difficulties in gaining infertile male respondents. The response/non-response rate for this study showed that for every agreement to be interviewed there was a matching refusal, thus illustrating a more general trend. Pursuing my own experience of gaining respondents, it was suggested that the whole process of producing a non-response rate is socially accomplished. This was illustrated by the case where

an intermediary had claimed to have contacted a candidate, who supposedly said he was not interested in being interviewed, however, there was a strong suspicion that the intermediary was lying. Obviously, the recognition of such a possibility involves everyday practical knowledge. Moreover, there does not have to be a dramatic or odd event for non-response rates to be socially accomplished: the research process called "interviewing", whether more or less structured, involves talking to people and inferring from the way they are talking, 'exactly' what they are saying - this is a perfectly ordinary everyday action. Hence, the messy work of social research cannot but be dependent upon common-sense procedures for linking accounts to realities, even though this dependence may not be readily apparent in the finished textual product of social science.

This 'seen but unnoticed' dependence upon common-sense was highlighted in the analysis of the accounts of non-response. Eight extracts were presented as data, and three main accounts of non-response were identified in the data corpus: 'sensitivity' with the subset account of the 'fertility-virility linkage'; 'children are not so important to men'; and 'pragmatic and other factors'. It was argued that these accounts were centrally concerned with the attribution of motives for non-response. Given that the extracts were from social science discourse, it could be expected that there would be a strong concern for the adequacy of, and grounds for, the statements made. But interestingly, the extracts displayed a wide range of motive attribution work: from the apparently more scientific model of 'no comment' when no evidence was available, to the grafting of traditional objective scientific methods onto taken-for-granted

inferences about infertility; and to an almost complete reliance on taken-for-granted knowledge of infertility in 'Everyone knows' declarative statements. In general, there was a strong presence of common-sense assumptions about infertility which flowed through to the motive attribution work. This should be no surprise: social scientists, as thoroughly enculturated members of the same culture they are studying, will, in their accounts of infertility and non-response, use public, held-in-common ways of understanding infertility and motives for non-response. To do otherwise would be to produce non-sense.

A central objective in this chapter has been to avoid presenting either, common-sense as faulty, or, social science as a fancified version of common-sense. That is, the analysis was descriptive, focusing on the intermixing of common-sense and social science in accounts of non-response. However, Owens' work, provided as a case study in the final analytic section, appears to provide good grounds for a more critical stance. Owens provided the most detailed list of explanatory factors for high rates of male non-response, but in the end, based on an appeal to what 'Everyone knows', he opted for a sensitivity account. By itself this does not present a problem; however, implicit in Owens' argument is a 'politics of experience'. The men who did speak to Owens commented only very infrequently that they found infertility a sensitive matter, or that they felt infertility threatened their virility. This presented Owens with a 'reality disjuncture': his reading of the infertility literature told him that men are threatened by linkages between their fertility and virility, but the infertile men that he talked to hardly ever said this. It is in the

resolution of this disjuncture that Owens resorted to a politics of experience. He discounted what the infertile men said, and favoured the views of the infertility researchers and the wives of the infertile men (who, Owens suggests, did provide some validating evidence for the fertility-virility linkage). In my view, there are good grounds to question whether the reports of infertile men should be so easily discounted and ironised by researchers. Such a blatant politics of experience should be avoided.

Before proceeding to the final chapter, which in many ways is relevant to the problem of irony, there is one further point of discussion. This concerns what can be called the 'seamless web'.

The Seamless Web

A point that has not been highlighted above, but should nonetheless be apparent, is the way that both the actions of those who do, and do not, respond, are incorporated in a mutually consistent and reinforcing manner in the broader explanation of infertility. For example, in the 'children are not so important for men' account, previous theoretical and empirical examinations of fertility and infertility were used to explain non-response, and in turn, albeit implicitly, accounts of the high rate of male non-response reinforce these theoretical and empirical examinations. Thus, to conclude this chapter it is worth looking at the way that accounts of non-response figure in the broader explanation of infertility and are incorporated in a 'seamless web'.

Heritage uses the phrase 'seamless web' in a discussion of "accounts and the structure of social action" (1988: 138-141, also see Heritage,

1984: 106-110). His concern is with the centrality of accounts in the reproduction of social order, and he starts by emphasising a basic point of ethnomethodology's 'procedural turn': shared procedures are both the means whereby actions are produced, and understood. Maintain these procedures (or methods) and both the social organisation of action, and the social intelligibility of action, are maintained. As an example, Heritage considers the simple rule that after a question a second speaker should produce an answer. Consistent with the above, this is both a rule of conduct and a rule of interpretation - the rule provides guidelines for temporal sequencing, and for how the action and its respective parts are to be interpreted. But, like all rules, it is not always complied with; this could be seen as an erosion of the rule's power, but this does not happen because an accounting machinery with important socially integrative functions comes into play.

Simply, the absence of an answer to a question brings with it two major types of interpretive option. Firstly, it could be reasoned that the rule that questions should be answered no longer applies to the current situation, and that some other yet to be determined rule applies. This option very rarely happens. Instead, secondly, we cling to the presupposition that questions should be answered, and therefore the result is that the absence of an answer requires explanation - an account is formed. In a logical sense, any number of accounts could be thought of, but the task is to find one that fits the circumstances, to offer the 'natural' account for the situation. In addition, accounts often have negative implications for the non-answerer, hence adding to the rule's power.

The crucial point to be taken from this relatively simple example is that the second interpretive option does not weaken the rule, but in fact embodies the presupposition that questions relevance answers. That is, as Heritage puts it, the 'exception proves the rule':

For speakers there are only two options: either the rule linking questions and answers is complied with and the question is answered, or the lack of an answer is an exception to the rule which requires some kind of 'secondarily elaborative' explanation. ... The exceptions with their explanations thus become 'the exceptions that prove the rule' because the provision of such explanations maintains the rule's presuppositional status both as a rule of conduct and as a rule of interpretation. Once again, we encounter a closed circle of interpretation. Presuppositional rules of action and interpretation interlock with the organization of account giving to form a seamless web - a self-motivating, self-sustaining and self-reproducing normative organization of action. (1988: 140)

In this way, ordinary everyday accounts of even the most mundane matters (eg. failure to return a greeting; failure to answer a question) make a crucial contribution to the maintenance of social order.

In many ways the 'seamless web' can be seen as a restatement of the "documentary method of interpretation" (Garfinkel, 1967), and it is mainly in this sense that implications will be drawn from it, as follows. While this thesis does not contain an overall review of the social science literature on infertility, it should be clear from the literature that has been discussed, that infertility is framed as a social and individual problem. So often academic work on infertility begins by stating that it is a major life crisis for individuals (see introduction to chapter four). This approach is clearly linked to the predominance of psychological studies of infertility that have considered both,

possible psychogenic causes of infertility, and the impact of infertility on psychological function (for reviews see, Edelman and Connolly, 1986; Unruh and McGrath, 1985). But, in addition, social scientists have studied infertility as a 'social problem', as the following list of research topics indicates: the 'social construction' of infertility as a matter of "desperation", "disease" or "social concern" (Gerson, 1989; Becker & Nachtigall, 1992; Scritchfield, 1989); stigmatisation, self-labelling, infertile identity, and coping strategies (Miall, 1986, 1989; Olshansky, 1987; Sandelowski, 1986, 1987; Woollett, 1985); the impact of infertility on marital and sexual relations (Greil, Porter, & Leitko, 1989); problematic aspects of doctor-patient relations within infertility treatment (Becker & Nachtigall, 1991; Meerabeau, 1991); provision of health services for the infertile (Henshaw & Orr, 1987; Pfeffer & Quick, 1988); the 'medicalisation' of infertility and negative impacts of reproductive technologies on women (see chapter three); and the epidemiology of infertility (Aral & Cates, 1983; Belsey & Ware, 1986). There are many more studies in this vein.

At risk of generalisation, this approach to infertility establishes the underlying pattern that infertility is a problem.¹⁴ It is not being argued here that this notion is a fabrication, a fallacy, a myth, etcetera; it is simply being emphasised that this general typification of infertility as a problem can be treated as an underlying pattern which informs the practical work of researching infertility in various ways.

¹⁴ I am not aware of any research specifically on the topic 'infertility', which does not make at least some reference to infertility being a social or individual problem in the way that the above research approaches 'problem'. Research of a more meta-theoretical nature on infertility seems to occur when 'infertility' is not the specific topic of research. An interesting example here is the work of Heritier-Auge (1989) on mythical notions of blood and semen.

Accounting for non-response is the case in point. It has been accounted for in a way which strongly reinforces the view of infertility as a problem, but this is despite the fact that non-response as an absence of information (although, as we have seen, some information can be made available by non-respondents or others), makes possible an indefinite number of accounts. For example, a perfectly plausible account of non-response is that the family pet died just prior to the request for an interview, hence the candidate did not wish to respond. It would be straining things to apply this as an overall account of non-response, nonetheless, it is logically plausible as a pragmatic account. But, the point is, we do not see this account precisely because it does not fit the circumstances in which the accounting work is being done, that is, the predominance of the view that infertility is a problem. Very clearly, the "documentary method of interpretation" is applicable here: the underlying pattern (infertility is a problem) provides the basis for finding and interpreting individual evidences (the phenomenon of high male non-response), and in turn the underlying pattern is derived and reconstituted from the individual evidences.

The interesting thing here is that using the same principle the following is equally possible. Take the case where researchers work with, or to, the assumption that male infertility is not a problem for men - call this assumption one.¹⁵ Presuming that these researchers

¹⁵ The following discussion is stimulated by Heritage's argument about the 'seamless web', but note that it is a speculative use of Heritage's argument. For one, I am referring to 'assumptions' and not 'rules', although the two are similar. More generally, social science accounts of infertility and non-response are much more complex than the relatively simple rule that after a question a second speaker should produce an answer. Nevertheless, my argument seems consistent with the general principle of Heritage's discussion.

set out to talk to infertile men, then we have the simple outcome of 'yes' or 'no' to requests for interviews. Response and non-response rates can then be calculated and made available for accounting work. Now, a good response rate, in itself, can be taken as confirmation of assumption one: infertile men do not find infertility a problem and concomitantly have no qualms in talking to researchers about their experience of infertility. That is, the assumption is being complied with. In the case of non-response, under assumption one it is perfectly plausible to offer the account that infertile men did not respond because the matter was so mundane and uninteresting to them that they could see little point in responding to a researcher. In Heritage's terms this would equate to the exception (non-response) proving the assumption, hence, response and accounts of non-response would form a 'seamless web' turning on assumption one.

Obviously, however, this form of explanation is absent in the data corpus - it does not even appear to have been entertained. Instead, response and accounts of non-response are quite clearly derived from, and constitutive of, a different assumption: male infertility is a sensitive matter. In short, it is a problem - call this assumption two. Here, response can be interpreted as compliance with assumption two by arguing, for example, that infertile males will talk to researchers because it has a cathartic effect - they are glad to 'get it off their chest'. MacNab's statement that "The need for a forum for men struggling with the problem is both evident and denied" (extract 7) seems related to such an argument. Equally, non-response is accounted for in a way that is consistent with the underlying assumption. Despite there being any number of plausible

explanations for why men would not want to talk to a researcher about their infertility, a sensitivity account is the most dominant and "intuitively plausible". Further, as Owens' work so clearly exemplifies, even if respondents' reports are directly at odds with assumption two, the assumption can still be maintained irrespective of the respondents' contrary reports, through a 'politics of experience'. This is a very powerful device for maintaining presuppositions or assumptions, and in this sense we could say, mirroring a well known phrase from Garfinkel's study of jurors' practical reasoning (1967: 104-115), that 'the explanation comes before the evidence', or, at least, that it stands irrespective of the evidence.

In this way, both response and non-response are incorporated into a 'seamless web': infertility is constituted 'for another first time' as a problem, as a sensitive issue. People who are approached by researchers wishing to hear about their experience of infertility, irrespective of whether they agree or disagree to be interviewed, thus

find themselves in a world of practical actions having the property that *whatever* they do will be intelligible and accountable as a sustaining of, or a development or violation, etc. of, some order of activity. ... [T]he actors' actions, to adapt Merleau-Ponty's phrase, are condemned to be meaningful.

(Heritage, 1984: 110, original emphasis)

Infertility researchers work within, and in relation to, the world of academic knowledge, which strongly guides them to study infertility as an individual or social problem. This underlying pattern is then brought into play in both, accounts of respondents' experiences of infertility, and, in accounts of the motives of non-respondents. Thus, non-respondents' actions become an exception that proves the

underlying assumption that infertility is a problem. The simple act of non-response is "condemned to be meaningful" as social scientists reconstitute infertility as a problem. Common-sense procedures are deeply embedded in the whole process; the whole process is a common-sense procedure.

Chapter Six

Conclusion: What's the Point?

6.1 *Introduction*

The four empirical chapters offered in this study are essentially stand-alone pieces. They do have general points in common, the use of ethnomethodological insights being the most obvious, but they are not intended to add up to a comprehensive sociological account of male infertility. Hence, this conclusion will not offer a summary view of 'what has been learnt' about male infertility. However, it is useful to reflect on the study as a whole by considering the general question, 'what's the point?'. This question has a history with respect to ethnomethodology, and discussion begins by clarifying the particular variant of it that is pertinent to this study.

By the history of the question, I am referring to the fact that a common reaction to early ethnomethodology was a terse, 'what's the point?' (Lynch & Peyrot, 1992: 118; Sharrock, 1989: 660). Typically, this stemmed from ethnomethodology's detailed attention to apparently trivial or mundane social action, and the way that analysis focused on form and ignored the content of the action. In the view of many sociologists there was little point to such analysis - it did not even look like sociology. Of course, counter-responses were made to this reaction, and there are now a good number of programmatic statements that make clear the inherently sociological nature of

ethnomethodological inquiry, clarify misunderstandings about it, and make the general point that form and content are mutually constitutive (eg. see, Button, 1991; Heritage, 1984; Hilbert, 1992; Peyrot, 1982; Sharrock, 1989; Sharrock & Anderson, 1986). And, as noted in the introductory chapter, there are a growing number of empirical studies which combine a traditional sociological interest in substantive topics, with insights from ethnomethodology.

The upshot of both these factors is that the sociological 'point' of ethnomethodology is no longer opaque or mysterious. As Pollner has put it in a nice metaphorical turn:

Ethnomethodology is settling down in the suburbs of sociology. There are signs of recognition, acceptance, and even welcome from the neighbors (Zimmerman 1987). ... To be sure, few want their children to marry an ethnomethodologist, much less to be one - and they rarely hire one. Nevertheless, the discipline recognises and begins to incorporate the contributions of what was once regarded as a pariah. (1991: 370)

However, as this piece also indicates, there is still some lingering doubt about ethnomethodology - most sociologists would not want their children to marry one, and they are rarely hired.

In my view, a good part of the reason for this lingering doubt stems from a more specific form of the 'what's the point?' reaction. Given sociology's long-standing concern with the topic of 'social inequality', and an almost sanctionable trend for sociologists to offer correctives to alleviate social inequality, the complaint can be raised that ethnomethodological studies 'do nothing'. Indeed, under the policy of "ethnomethodological indifference" (Garfinkel & Sacks, 1970) offers of correctives were explicitly avoided. The argument being, that if

any social scene is reflexively constituted, there and then, as orderly and sensible, the point is to study the practical reasoning procedures involved, **without** 'taking sides' as to who has the correct description of the events.¹ Thus, even if the earlier form of the 'what's the point?' reaction is now heard only infrequently, the variant reaction, 'what's the point? - it does nothing', is still common.²

Again, this more specific reaction has not gone without comment and discussion from ethnomethodologists. Some have argued that ethnomethodology can take on a critical role, while others have argued that it was never designed for such work, for good theoretical reasons, and it should not begin to attempt it now. Given the existence of a general debate on this issue, there is the possibility of reviewing and weighing-up these different arguments. However, this approach will not be taken; instead, I wish to take a closer look at a single debate between three proponents of ethnomethodology. This debate centres on the issue of description and critique in social science, which clearly is central to the 'what's the point?' issue. Moreover, the debate provides a useful platform for reflection on this study's four empirical analyses, and, as will be seen, this ultimately leads to a relatively optimistic conclusion about the study. While the

¹ Moving the focus of investigation from the 'correctness' of everyday assertions to how they are possible and intelligible is clearly a central tenet of ethnomethodological investigation. Sacks in particular stressed this focus early in the development of ethnomethodology (Sharrock & Button, 1991, fn. 24, p. 174).

² Hereafter, when the phrase 'what's the point?' is used it is the second variant that is being referred to, that is, 'what's the point? - it does nothing'.

debate is of interest in itself, this latter section of the chapter provides the central reason for discussing the debate.³

6.2 *Description and Critique: the debate between Bogen and Lynch, and McHoul*

Recently the *Journal of Pragmatics* presented a debate with an apparently simple two-part format. First, a comment by Bogen and Lynch (1990) on an earlier paper by McHoul (1988a), and second, a reply in the same issue by McHoul.⁴ This apparently simple format belies the complexity of the debate. The initial paper by McHoul was in turn an extended critical review of Jeff Coulter's substantial body of work on language and the sociology of mind. Thus: McHoul critically reviews Coulter; Bogen and Lynch offer critical comments on McHoul's review; and then McHoul replies to Bogen and Lynch. As McHoul comments "Who, then, can tell what the rules are here? - who is addressing whom in what sequence about what?" (II: 523). Despite this initial complexity, it is clear that the debate is concerned with the general problem of what social science should do, and this is approached by considering the specific case of ethnomethodology. The overall question is, what mixture or separation of the elements 'social critique' and 'description' should ethnomethodology achieve.

³ Note, though, that explication of the debate takes up more space than reflection on the study. However, as the two sections are closely connected and the explication of the debate allows a quick reflection on the study, this imbalance should not present a problem.

⁴ Below, there will be substantial quotation from the debate, and since it comes as a two-part package, referencing will proceed as follows: (I: page no.) references Bogen and Lynch (1990); and (II: page no.) references McHoul (1990).

The participants to the debate all have a high standing in ethnomethodology,⁵ making it clear that this is a debate of some import within ethnomethodology - Jayyusi (1991: 247) has labelled it as "perhaps the most salient example" of debate about the role of political critique in ethnomethodology. Below, consideration is given first to Bogen and Lynch's critical comments on McHoul (1988a).

Bogen and Lynch's Position

Although McHoul's review of Coulter's work is extensive (and generally very positive), Bogen and Lynch make it clear that they will be focusing on only one aspect of McHoul's review. As they put it in the first sentence, "we will be considering recent arguments advanced by Alec McHoul (1988) to the effect that ethnomethodological and Wittgensteinian lines of inquiry hold forth the promise of providing a rigorous analytic basis for the socio-political critique of 'discourse'" (I: 505). In short, they find McHoul's attempt to link ethnomethodology with a 'political turn' to be "ill-fated". Since McHoul's reply is addressed specifically to Bogen and Lynch's representation and attendant critique, it is this explication, rather than McHoul's initial argument, which will be summarised.

⁵ Lynch, with links to Garfinkel both in terms of training and research collaboration, and with his seminal studies in the social study of science (see, Lynch, 1985; Lynch, Livingston & Garfinkel, 1983), is recognisable as a leading contemporary ethnomethodologist. Bogen is more a newcomer, having only recently completed his PhD, but he has already published a number of ethnomethodological studies and his placement at Boston University along with Lynch and Jeff Coulter, suggests high regard within ethnomethodology. In contrast McHoul is much more eclectic in his field of study, with his early work including standard conversation analysis (1978), theoretical defence of ethnomethodology (1981), and an important ethnomethodological investigation of texts and reading (1982), but with his later work branching out to the more general field of communication studies (eg. 1983). Despite this eclecticism there is no doubt that McHoul is recognised as a very competent ethnomethodological researcher.

From the start, Bogen and Lynch cast McHoul's project as one of assimilating two poles in social science: on one hand, detailed empirical investigations of social praxis, and, on the other hand, social critique. They see McHoul's positive reading of Foucault as the major reason for this assimilation project: he takes from Foucault the important point that academic disciplines are not outside of discursive formations, but are indeed 'disciplines' which have direct and indirect power-effects. The upshot is that academic neutrality is now seen as an impossibility -after Foucault one can no longer remain neutral about discursive practices, and this includes the discourse of members and academic analysts.

McHoul's agreement with Foucault on this point sets up an initial problem of incompatibility with the policy of "ethnomethodological indifference". Within conversation analysis, for example, it is argued that if conversation has an endogenous order, then it is legitimate to analyse that order irrespective of any concern for social inequalities, or the possibility of external structures enabling or constraining the talk-in-interaction (eg. Schegloff, 1987). McHoul accepts the argument that conversation displays an endogenous form of organisation, but does not accept that this necessitates "indifferent" analysis.

Bogen and Lynch admit that his argument here is a challenging one. McHoul suggests that the devices which ethnomethodological analyses of language-use identify - turn-taking rules, sequential combinations, and so on - are also used by institutionally

subordinated subjects for resisting power. Certainly, institutional power acts as a skewing force, or limit, on actual social interaction and language use,⁶ nevertheless, given the dynamic nature of interaction, in the actual use of institutional conversational machinery there is room for resistance by subordinated subjects. As Bogen and Lynch put it, McHoul has identified the everpresent "*potential* difference between the 'legitimate options' for action provided by the local organization of interaction, and the skewed distribution of options in institutionalized uses of interaction" (I: 508, original emphasis) as a resource for political struggle. But more importantly, McHoul suggests that fine-grained empirical analyses can uncover such struggle, and hence academic analysis can be a basis for social critique.

This argument is made clearer by following through an example that McHoul provides. This consists of a re-analysis of some ordinary conversation that Coulter had analysed, and since it is important in McHoul's (1988a) argument, and Bogen and Lynch also focus on it, it is as well to reproduce the excerpt here. The transcript is of a tape-recorded interaction between a 'Mental Welfare Officer' (MWO), a 'prospective patient' (PP), and the 'patient's wife' (PW):

- "1 MWO: Dr ... Dr K. asked us to call ... to take you to hospital.
- 2 PP: Err ... I'm alright as I am.
- 3 MWO: You know Dr K. and Dr. S. that saw you last night-
- 4 PW: -Yes-

⁶ This occurs primarily through pre-specified turn-taking procedures and limits on types of action done in turns. Interaction in the army, courtrooms, and medical consultations are obvious examples.

- 5 MWO: -they want you to go up this afternoon, and they've asked us to call ... we've got the car with us, you know-
- 6 PP: -Aah, I, I'll make it in my own time//if you don't mind.
- 7 PW: //ya can't make it in yer own time.
- 8 PP: Course I can. (Pause of 1.5 secs.)
- 9 MWO: Err, well, you know, I mean Dr K.'s quite busy and he's made an appointment for you this afternoon at the hospital//()
- 10 PW: //no harm to go and see him is there?//()
- 11 PP: //no, I-I'd rather go on me own I ... I
- 12 MWO: Won't take us long down the motorway in the car ... go up the M55. Be there in no time.
- 13 PP: Nah I'll remain as I am (Pause of 2.0 secs)
- 14 PW: Ya can't remain as y'are ya gotta see the doctor ...
- [// marks point at which overlap commences]."
- (McHoul, 1988a: 376-377, quoting Coulter, 1979: 26)

McHoul's re-analysis of this transcript emphasises the "conceptual contestation" occurring in the interaction: while the MWO is trying to 'bring off an incarceration', the PP, through the conversational interaction, succeeds in getting the outcome heard as simply a lift to the hospital. This is despite the common knowledge that asylum admissions of this sort are not a matter of choice.

There is nothing particularly stunning about pointing out that people contest one another's actions, but when considered within the context of how ethnomethodology analyses ordinary conversation, Bogen and Lynch suggest that McHoul's argument represents a fundamental challenge to ethnomethodological naturalistic inquiry. McHoul argues that when an analyst identifies an 'ordinary conversation', the term is a gloss for what is always and already an area of political contestation. Moreover, this political contestation is over factors endogenous to the interaction, rather than over factors exterior to the

interaction (eg. gender, class, etc.). The point of McHoul's re-analysis of Coulter's transcript is to show that the PP worked to get the putative incarcerative act heard as an everyday conversation, and hence recast the terms of the interaction. As Bogen and Lynch put it:

To say that the above instance is identifiable and analyzable as an *ordinary conversation* is, in McHoul's account, to take an ethical stance in support of PP's 'right' to refuse to go along with the MWO.

Such a 'right' is included within the optionality system of the adjacency pair organisation of conversation. Although refusals of offers may be 'dispreferred' ... they are none the less a 'legal' option for recipients of offers in conversation. Presumably, the work of "bringing off an incarceration" in the above instance includes no such option, so that what is legitimate and logical *for conversation* is transgressive *for incarceration*.

(I: 510-511, original emphasis)

Thus, McHoul's challenge to ethnomethodological approaches to language-use does not question the well established incarnate orderliness of conversation, but builds upon it to ask questions about how the very relevance of ordinary conversation for interaction is accomplished for the case being studied: how is the system of rules of conversation legitimately put in place? As McHoul tries to show from his re-analysis of Coulter's transcript, consideration of this question involves a move from naturalistic inquiry to a critical pragmatics of language-use (hereafter, 'critical pragmatics'). On this view, fine-grained analyses of ordinary conversation can be used to expose the political contestations occurring in, and as, talk-in-interaction.

It is clear that Bogen and Lynch recognise McHoul's argument as an important and challenging one, nevertheless, they do not find it convincing. Their critique is twofold: first, they identify some problems with McHoul's re-analysis and recommendations that similar analyses might be the basis for a critical pragmatics; and second, they return to the more general issue of what ethnomethodological description aims to do, suggesting that McHoul's dissatisfaction with it is misplaced and based upon several misunderstandings. The two parts to their critique are explicated below.

Briefly, Bogen and Lynch make four "obvious criticisms" about McHoul's re-analysis and recommendations. First, regarding McHoul's suggestion that the PP had the interaction successfully "heard" as the offer of a lift to a hospital, they suggest that there may be no connection between how the interaction is 'heard' and the eventual outcome. Choices may indeed be offered in 'bringing off an incarceration', but this may make no difference to the eventual outcome - the social control authorities may presume the ultimate outcome but leave the actual details of how it is effected to the prospective patient. Second, there is the issue of just why political critique should be based on analyses of conversation. It is possible that critical pragmatics could counteract institutional power, but surely there are more obvious and practicable techniques, for example, lobbying for patients' rights legislation. Third, Bogen and Lynch comment that, "in any event, it seems that PP is quite able to exploit the options presented by the local interactional situation without having to be instructed by academic analysts" (I: 513).

Fourth, they note that while it is clear McHoul endorses the notion that pragmatic analyses of language-use ought to do something, he provides nothing of substance about how it should do what it ought to - there is no attention to the appropriate means whereby critical findings might be applied to real-world social praxis. Without such details, Bogen and Lynch conclude that McHoul's suggestion "looks like a mere aggrandizement of academic critique" (I: 514).

Bogen and Lynch continue in this openly critical manner in their discussion of McHoul's treatment of ethnomethodological description. The discussion here is broad-ranging moving from an outline of Wittgenstein's philosophy, to problems of 'irony' in social science. However, in this complex discussion there is one main point being made: McHoul is mistaken in trying to link 'descriptivism', as formulated by Coulter and in ethnomethodology, to forms of social science which make socio-political critique a primary aim.

Central to this argument is discussion of the policy of "ethnomethodological indifference".⁷ Here Bogen and Lynch spend some time outlining Wittgenstein's approach to philosophy, quoting favourably his programmatic statement that "We must do away with all *explanation*, and description alone must take its place" (I: 515). They emphasise that such a stance did not evolve *ex nihilo*, but clearly involves a fundamental shift from well established philosophical practice: the key to understanding disinterested analysis is to put it in relation to what it is disinterested in, or indifferent to,

⁷ Bogen and Lynch reference Garfinkel and Sacks' (1970) introduction of "ethnomethodological indifference", but their discussion mixes the terms "indifference" and "disinterested analysis".

and that is philosophical or sociological explanations of language/language-use. Thus, disinterested analysis does not imply "*blanket* indifference to, and hence, *total* detachment from, the normative commitments intrinsic to the analytic 'object' (viz., language)" (I: 516). Instead, it represents a "reasoned indifference" to explanatory modes of sociological and philosophical inquiry. Also, indifference is not to be equated with 'objectivity' -there is no claim that disinterested analysis is of necessity correct because it affords a privileged stance on reality.

Having attempted to clarify the matter that indifference is posited in relation to avoiding causal explanatory theorising, and is not another attempt at objectivity, Bogen and Lynch move on to McHoul's main problem with indifference. Simply, that after Foucault and Derrida indifference is no longer feasible: pragmatic analyses of language-use now have to show that they have something critical to offer - they ought to do something. The trouble here, Bogen and Lynch argue, is that if disinterested analysis is dropped then one can only return to the traditional problems of 'explanatory theorising', primarily that of irony. They suggest that the traditional sociological approach to the heterogeneity of social action is 'methodological irony': deciding in advance how interaction will be treated; implicitly aligning with one or another participant's reading of the social event. For example, in McHoul's re-analysis he sided with the PP and valorised his practices.

Bogen and Lynch argue that McHoul wants to retain ethnomethodology's emphasis on fine-grained analysis, which aims

precisely to minimise problems of irony, while retaining a commitment to political analysis. But the two cannot be reconciled:

Although he subsequently argues that "(s)uch an analysis does not necessarily entail a methodologically ironical position" (1988: 379), what he fails to notice is that by valorizing 'transgressions' he implies exactly this, *irony*, but now in the name of a presumably progressive political platform.

... McHoul operates under the mistaken impression that politically interested analysis represents a progressive 'turn' in sociological and linguistic thinking. In contrast, we argue that such a 'turn', however much inspired by contemporary theories of discourse and social action (eg., Habermas and Foucault), represents a return to a very familiar form of intellectual undertaking, and finally, that for all its critical pretensions, McHoul's attempt at constructing a *post-conventional, politicized theory of discourse* fails to move beyond the traditional aporias of social critique. (I: 518, original emphasis)

Bogen and Lynch do not detail what the "traditional aporias" are, but it is clear from their discussion that they are referring to the related problems of 'irony' and 'constructive theorising'.

Overall then, Bogen and Lynch thoroughly reject McHoul's formulation of critical pragmatics. They finish by claiming that their's is not a "despairing conclusion": descriptivist analyses of ordinary action can have "more limited 'therapeutic' import"⁸ (I: 519), and furthermore, their critique of 'explanatory theorising' is of value in itself.

⁸ Bogen and Lynch do not detail what they mean by this, but they seem to be referring to the ability of ethnomethodological studies to offer practical implications for real world praxis. Atkinson and Drew's concluding chapter - "Postscript: Notes on Practical Implications and Possibilities" - in their book on interaction in courts (1979) is a very good example. See the list in chapter one of studies which engage ethnomethodology with conventional sociology for other good examples.

McHoul's Reply

The title to McHoul's reply - "CRITIQUE AND DESCRIPTION: An analysis of Bogen and Lynch" - indicates his intentions. The topic is offered as critique **and** description, not critique **or** description, which Bogen and Lynch seem to prefer; and, McHoul calls his reply an "analysis", which we find means a 'deconstruction' of Bogen and Lynch's underpinning assumptions about the social sciences.

McHoul suggests that Bogen and Lynch's primary underpinning assumption is that social science has a twofold division of labour. Supposedly, one program produces rigorous descriptive analyses with no social critique, while the other produces explanatory theoretical schemes accompanied by social critique. The former engages in critique only of the latter program, essentially over its explanatory mode. The first point that McHoul makes is that it is debatable whether such a division of labour actually exists. At least, the two poles are far from independent, for in Bogen and Lynch's model of social inquiry "the descriptivist program ... depends for its very existence on the explanatory program yielding mistakes, ungrounded theorisations and the like. Its purpose would be lost without its foil" (II: 524).

McHoul's main point of reply is that this binary is in need of deconstruction. He is definitely committed to the view that it ought **not** to exist, and this stems from his acceptance of post-structuralist arguments regarding language and power. Part of the process of deconstruction that he recommends "has to do with showing that to

speak or write within any social scientific form of life - descriptive, explanatory or otherwise - is to engage practically with what Bogen and Lynch call "disciplinary power 'at large'" (1990: 519)" (II: 524). Accepting the argument that academic disciplines are entwined with and within the operation of these types of power, leads McHoul to reject the separation of description and critique. What he proposes is not so much a synthesis of the two, or their recombination at another level, but a simple recognition of their mutuality:

... description and critique are mutual ends since (i) description cannot be performed without certain bedrock theoretical assumptions none of which are, *a priori*, beyond *critique* - an example being Bogen and Lynch's own assumption about the division of labour in the social sciences and (ii) critique can have no point if it remains purely transcendental; that is, unless it gears into the complex and specifically 'worldly' matters which *empirical descriptions*, such as those of ethnomethodology itself, afford. (II: 525, original emphasis)

Thus, the interdependence of description and critique is contrasted with Bogen and Lynch's binary of description or critique.

Making this point takes up the first three pages of the nine page reply, after which McHoul moves to the issue of indifferent analysis. Consistent with his deconstruction of the critique or description model, he argues that social scientists must now take serious heed of Foucault's descriptions of the unintended power effects of disciplinary knowledge. He notes that many social scientists are indeed attempting to calculate the resistive and emancipatory potential of their own disciplinary knowledge, or at least trying to gain more control of the way that knowledge can be used to legitimate existing modes of surveillance. His point here is that ethnomethodology needs

to take heed of this trend, as even the "most dogged descriptivism" is not immune from power-knowledge effects.

This argument is not substantiated to any degree, nevertheless, several points are mustered in general support. There is the continual acknowledgement in early ethnomethodological studies of grants from the U.S. armed forces, and McHoul's own experience of being asked for reprints of his conversation analytic work by medical, military, psychiatric, governmental, and financial organisations. At a more chilling level, McHoul cites the case of the South African corrective services asking for a copy of his paper on interrogation (McHoul, 1987c). In light of such events McHoul suggests that at a practical level the notion of 'indifference' is a dangerous luxury.

Bogen and Lynch's other main argument, that 'indifference' is not about 'blanket indifference' but specific indifference to the truth claims of explanatory theorising, is also quickly dispatched. McHoul argues that this is based upon a false conception of the difference between ethnomethodology and other social science paradigms. Natural or literal description is just as inaccessible to ethnomethodology as it is to any other form of sociological inquiry. All social science paradigms constitute their 'object' in their procedures of study, and ethnomethodology is not immune from the problems that it identifies in explanatory theorising.

However, the impossibility of literal description should not be taken as a reason to end analysis or to retreat into nihilism. Instead, inquiry into how social science is actually accomplished becomes a part of

critical pragmatics. For McHoul, critique is directed precisely towards self-reflection upon the relationship between theorising and the objects of theorising, whether this is 'society' or more localised interaction. And this self-reflection is far from an "aggrandizement" of social science, as Bogen and Lynch claimed. It is on this point that McHoul clearly elaborates his envisaged program, which is worth quoting at some length:

... the idea of a political linguistics or, more generally, a critical descriptivist program in the social sciences, could only ever mean a highly minimalist and piecemeal realisation that social science is always part of and imbricated in whatever 'social' it attempts to describe or otherwise articulate ... A good ethnomethodologist, in my book, would want to ask 'how'? - how does this articulation take place? The very idea that social scientific accounts, including ethnomethodological accounts, are irrevocably incarnate in the social practices which they account, is to me essentially the point of Garfinkel's (1967) ethnomethodological principle of reflexivity: except that for me it must operate at other levels than the merely 'methodological'.

If the above holds at all, it may be possible to see that the debate between Bogen and Lynch and myself instantiates a point which I tried to make in 'Language and the sociology of mind': namely that the level of 'bedrock assumption' (or 'depth grammar') in any form of life can itself be contestable. ... If the level of logical grammar ... is *not* contestable then we run the risk of invoking an absolute category such as Language with a capital 'L'. ... And so what I am proposing is no more and no less than a radicalisation of ethnomethodology's commitment to the piecemeal - so that it obtains not merely in empirical social relations but also at the level of the logical-grammatical 'agreements', 'conventions' or 'contracts' which may underpin those 'surfaces' (McHoul (1986)). (II: 527)

In short, McHoul takes a central premise of ethnomethodology - social facts are socially accomplished from within social settings - and argues that this must be applied to the study of the 'bedrock assumptions' underpinning social action. If these underpinnings are

conceptualised as accomplished, then, equally, they are contestable. Hence, we have a space opened up for critique - by members and analysts - of both surface action and underlying assumptions.⁹

Of course, this argument comes up against Bogen and Lynch's criticism that if members can perform such critique for themselves, they have no need of instruction by academic analysts. In response, McHoul is remarkably frank. After arguing that critique must mesh with complex real-world matters known via empirical descriptions, he comments that "I would even be prepared to concede that my paper on Coulter came nowhere near actually instantiating the position in practice if it were conceded, on the other side, that the position is sound in principle" (II: 525). This does seem akin to an admission that Bogen and Lynch's four critical points about his re-analysis, as an example of the proposed critical pragmatics, are cogent. However, while McHoul does admit the infancy of critical pragmatics, hence the cogency of Bogen and Lynch's points, he does provide some strong pointers as to how it might proceed. Specifically, he cites Foucault's work with a prisoners' information group, emphasising in particular the aim of avoiding speaking for others. Thus, contrary to Bogen and Lynch's somewhat uncharitable remark about academics telling people what they already know, McHoul favours an opposite principle: "more a broadening of the circulation of minor discourses ... than an 'importation' of theoretic knowledges *into* marginal locales" (II: 528, original emphasis).

⁹ McHoul's reference to studying 'underlying assumptions' should not be equated with 'realism' within social science. In an earlier paper (1987b) McHoul suggested the phrase 'anti-realist materialism' as another way of understanding ethnomethodology, and clearly the rejection of realist approaches to language-use is maintained in his vision of critical pragmatics.

McHoul concludes his reply by reiterating a belief that social science needs to show, publicly, that it has a form of critique to offer. For him this means showing three things: first, that it is not simply and unwittingly part of the technologies of power; second, that it can find a way to describe dominant social understandings and can construct social understandings outside the dominant; and third, that it has something critical and emancipatory to offer, even if the actual details of delivery are unformulated or sparse. The spirit of these three aims is probably commonly invoked within social science, however, what distinguishes McHoul's desired program is his insistence on ethnomethodological description as a base for critical pragmatics:

... this [form of critique] cannot be achieved without the plausibility, the analytic precision and the attention to empirical detail that Coulter and some of his colleagues have pioneered and so successfully demonstrated in their work as a *technical* end in its own right. ... [Thus] it seems sensible (to me at least) to recognise ethnomethodology for what it is, if an analogy from physics can serve: the most exacting and exciting approach we have to discursive 'particles', while remembering that approaches so far to 'wave' and 'field' are still comparatively inchoate and rudimentary at best, and often not even that. Hence my compulsion towards them. (II: 530-531, original emphasis)

6.3 *Reflection on the Debate*

To recap, Bogen and Lynch argued that McHoul's suggestion of reconciling descriptivism with socio-political critique is misplaced because the two are mutually incompatible. In reply, McHoul

deconstructed the critique or description binary and argued that the aim of producing socio-political critique is not a whim but a necessity for social science, and one which can proceed from a descriptivist beginning. Thus, the debate features two strongly argued opposing positions. Given this, it almost seems natural to pronounce a victor, but there are good reasons to downplay such adjudication. Chief amongst them is that the position of critical pragmatics is very much in the development stage, making judgement premature. In addition, reflection on the debate suggests that while there are obvious differences between the participants, there are also strong points of convergence.

At the core of the debate there is a central and pervasive agreement. Both parties adhere to ethnomethodology's central methodological policy that social research should proceed by precise analytic description of real-world social activities. Fine-grained description remains the cornerstone for Bogen and Lynch's, and McHoul's, program of inquiry. The point of stubborn difference is the issue of whether description can be extended into critique. But, it should be noted that this is not a debate about an actuality, but about a potential. This is clearly admitted in McHoul's comment, "I would even be prepared to concede that my paper on Coulter came nowhere near actually instantiating the position in practice if it were conceded, on the other side, that the position is sound in principle" (II: 525). This admitted lack of an instantiated program is a very good reason for avoiding pronouncing a victor.

However, one can certainly comment on the general principles being argued, and here I have to agree with McHoul that the position of critical pragmatics is "sound in principle". Despite the strength of Bogen and Lynch's arguments, particularly their critique of McHoul's re-analysis of the 'incarceration' interaction, in my view, McHoul's argument is more convincing. In principle, why should not ethnomethodology be used as a basis for critical inquiry? Whatever one makes of Foucault's work, there is little doubt that his arguments about the effects of disciplinary knowledge have been widely discussed within academia and have resulted in much self-reflection by academics. McHoul's program is obviously sensitive to such concerns, and his general view is no more neatly summed up than by Roy Turner's statement that "Ethnographers who have read Foucault lose their innocence" (1989: 13). Clearly, McHoul is no innocent, indifferent, ethnomethodologist; but the point of making his view public is not self-aggrandizement, but to argue that other ethnomethodologists should also lose their innocence.

If ethnomethodology can be thought of as ethnography of our own cultural practices, then, in the light of Foucault, to claim production of 'indifferent descriptions' of cultural practices while also eschewing socio-political critique, seems paradoxical. And one does not need to agree with Foucault to reach the same conclusion. As Jayyusi (1991) notes, ethnomethodology has always emphasised the mutual embeddedness of the conceptual, the practical, and the moral. Such an emphasis is well suited to critical analysis: "in offering detailed descriptions of the ways that practices get constituted locally as politically or morally relevant, she/he [the analyst] is involved,

minimally and unavoidably in *laying bare the moral significance of these practices as these are made available in our culture*" (Jayyusi, 1991: 249, original emphasis). To offer such inquiry while holding to a notion of 'indifference' does seem a dangerous form of innocence, there being no better example than the request by the South African corrective services for a copy of McHoul's paper "Why there are no guarantees for interrogators" (1987c). It seems unlikely that the request arose from a mere academic interest in conversation analysis.

However, having sided in principle with McHoul's argument it must be reiterated that an ethnomethodologically based critical pragmatics has yet to 'deliver the goods', as McHoul frankly admits. That this 'delivery' will be difficult is suggested by at least two points. First, McHoul began publicly formulating the program as early as 1984 ("Being seen to read the signs"), and has continued recommending it since (see 1986, 1987b, 1987c, 1988a); but despite this amount of time for refinement, the program has not progressed significantly beyond statements of intent. Second, similar arguments by others have also remained at a programmatic and anticipatory level (eg. Gubrium, Buckholdt, & Lynott, 1982; Gubrium & Holstein, 1987: 777; Hilbert, 1992, ch.10; Jayyusi, 1991: 246-249; Mehan & Wood, 1975: 218-224; Pollner, 1991), and extant empirical analyses which share the same spirit as critical pragmatics, have been disappointing (eg. Hartland, 1989; Smith, 1983, 1990). To put it briefly, Bogen and Lynch's "traditional aporias" - irony and constructive theorising - do seem to present a perennial problem for the development of ethnomethodologically inspired socio-political critique.

6.4 *Reflection on the Study in Light of the Debate*

The above admittedly brief reflection on the debate will suffice, for, as noted in the introduction, the main reason for explicating the debate is to consider the 'what's the point?' issue as applied to this study.

One immediate question arises from the discussion of the debate: if I have endorsed McHoul's vision of critical pragmatics, why is it that this study looks more like Bogen and Lynch's version of ethnomethodological inquiry? On reflection, it appears oriented to descriptivism and not critique: the emphasis is on criticism of explanatory theorising, and alternative empirical analysis, and not offers of explicit socio-political critique or correctives for real-world problems that might be associated with male infertility. Consideration of this puzzle provides a useful endpoint for this study. To precis the argument, it is suggested that my endorsement of critical pragmatics is not so much a paradox, but more a reflection of the similarities in the views of Bogen and Lynch, and McHoul. Below, the empirical chapters will be discussed in chronological order in relation to the points made about the debate.

In chapter two the question of how people disclose their infertility to others was considered. The foil here consisted of a dramaturgical model and the beginnings of a Foucauldian confession model of infertility disclosure. The former began with theoretical constructs

like 'face' and explained disclosure by individual motives and intentions linked to face concerns; the latter framed infertility disclosure in terms of constant societal pressures on individuals to confess their everyday illnesses and troubles. Against the emphasis of these models on factors which are anterior to talk-in-interaction, following Sacks I argued that a prior analytic task is to understand the kinds of devices that people have available to disclose infertility. This approach is based on the argument that the use of conversational machinery is itself a normative sanctionable matter -talk about experiences (infertility disclosure) has its own powerful endogenous structure. Although my analysis was speculative due to data constraints, it was sufficient to highlight the efficacy of a conversation analytic (ie. descriptivist) approach to infertility disclosure.

Clearly, the argument in chapter two does not offer direct critique of social practices involved in disclosures of infertility. Nevertheless there are two points worth noting here. First, although the possibility was not pursued, a full-fledged conversation analysis (using 'naturally occurring' data) of infertility disclosure, could be of 'therapeutic import' for people experiencing infertility. To know something about the form that disclosure talk takes should be a prerequisite for the realistic design and direction of practical actions for changing the experience of infertility disclosure.¹⁰ Second, following Bogen and Lynch, it can be emphasised that my descriptivist-based critique of explanatory theorising is of value in itself. If my argument is cogent,

¹⁰ Of course, this assumes the identification of factors to be changed - this identification itself requires consideration of empirical materials.

then it suggests that any corrective actions based on either the dramaturgical or the confession model will have an uncertain foundation. This is not to suggest that practical actions based on conversation analysis would be infallible, but it is to emphasise that if one is concerned with a conversational activity, which infertility disclosure is, then one should consider the dynamic constraints and resources that conversational machinery itself provides, rather than neglecting this in favour of external societal structures or quasi-psychological motives and intentions.

Chapter three (The language of reproduction: Is it doctored?) presented as a foil three examples of feminist arguments about the language of reproduction (or more specifically the description of male (in)fertility). Socio-political critique was absolutely central to these studies, the general argument being that the language of reproduction is doctored to further the subordination of women and the superordination of men in society. In critique, a standard ethnomethodological insight was used: language should not be treated as merely a secondary site where the effects of societal structures can be found, for language is a social practice in its own right. The three arguments by the feminists used various extracts from medical discourse as a site to find the workings of patriarchal inequality; in critique the point was made that this approach neglected the local socially organised nature of medical discourse. On this view, talk of medical discourse being 'doctored' is only made in the sense that medical discourse is produced by and for doctors, and assumes and enacts the background knowledge of their existential world.

"Ethnomethodological indifference" figured strongly in chapter three. It was noted that the whole area of reproductive technologies is saturated with socio-political issues, but no direct comment was made on these issues. Instead, my approach involved a critique of feminist explanatory theorising which highlighted specific misconceptions in the empirical analyses of the language of reproduction. This 'negative' approach is one area where the aforesaid paradox seems to 'bite hard': given the endorsement of critical pragmatics, why did I not attempt both description and socio-political critique of the practices of reproductive medicine? There are many factors which could be mentioned in answer, predominant amongst them being that such a study would be a major undertaking in its own right, hence, beyond the scope of this study. Simply, all I want to suggest is that in this case a critique of explanatory theorising was in order, and useful. Given contemporary views about the relationship between language and society, analysts need to be reminded that it is poor analytic practice to treat language as merely a reflection of other structures. The feminists took this approach because of an overarching desire to critique the external structure 'patriarchy' (or some reflection of it), but if it is agreed that language-in-use has its own social organisation then to approach the language of reproduction for the way it supposedly bears the marks of patriarchal society, is to risk a dangerous gloss of its first order social organisation. Surely, socio-political critique must connect somewhere with this first level of socially organised practical action. Hence, there is some value in first attempting to describe the socially organised nature of medical discourse.

In chapter four, the scope of study was broadened from an aspect of male infertility to that of humour, but examples of humour connected to male (in)fertility were used in the empirical analysis. Here the foil for discussion was Mulkay's study of humour, particularly his argument that humour could be a "potentially liberatory force" in society. Mulkay founded his argument upon Schutz's concept of 'multiple realities', arguing that humour establishes realities alternative to that of the serious world. Ultimately, Mulkay argued that humour served an essential function within society because it provided an escape from the onerous duty of maintaining the seriousness of reality.

The critique of Mulkay was based upon descriptivist principles. Following Sharrock and Anderson (1991), it was suggested that Schutz's comments on multiple realities could be read in a different manner: not for any claims about the existence of serious and humorous worlds, but to lead to a focus on the sequentiality of everyday life. This focus moves the study of humour away from humour as a competitor to serious discourse, to humour as a serious accomplishment. If members are treated as inquirers into their own culture, then humour can be seen not as a "liberatory force" but as discourse which provides important information about common cultural practices. Humour partakes of the seriousness of any practically accomplished situated conduct; the analytical task is then to describe the particularities of how it is accomplished. Several empirical analyses were made, consistent with these points.

Summarising so far, chapters two, three, and four all displayed a two part structure: in the first part, explanatory theorising was explicated and criticised, while in the second part a contrasting analysis based upon ethnomethodological descriptivism was presented. The degree of socio-political critique offered in the explanatory theorising ranged from a more individualist viewpoint in the dramaturgical model of infertility disclosure, to a clear structural power model in the feminist argument on the language of reproduction. Regardless of this range, they are all committed to a view of social science which 'takes sides' and formulates correctives. Regarding the paradox noted above, three simple points can be emphasised: i), the criticism of the explanatory theories should not be read as a wholesale rejection of the aim of socio-political critique itself, as the criticisms are more specifically addressed to theorising which is insufficiently grounded in precise consideration of the endogenous orderliness of social interaction, and, ii), nor should the criticism be read as an indication of belief in a twofold division of labour in social science (description or critique), instead, iii), the analyses reiterate fine-grained description as a worthy goal in itself, where this does not preclude the possibility that such fine-grained description might be part of socio-political critique.

While there is one further point I wish to develop in this conclusion, my feeling here is that these three points are a sufficient and succinct reflection on the study and the critique/description issue. Perhaps the cautiousness inherent in the three points invites the negative comment that nothing much has really changed. But such a comment forgets that simple description is not simply accomplished - as Sharrock notes

in a discussion of the way ethnomethodology has been received by other sociologists,

Those who have no interest whatsoever in the analysis of research materials and have never seriously attempted their careful (often unmotivated) inspection are unlikely to recognise, let alone, appreciate the sense of accomplishment available to those who try to analyse such materials in an intensive and close way, when they manage to give their materials as scrupulous a treatment as they can. (1989: 660)

While not everyone will recognise the amount of work that goes into simple description, and despite the still-present possibility of a terse 'what's the point?' reaction, the good news is that the sense of accomplishment available from fine-grained description is tangible and persistent.

A further point of optimism can be drawn out in relation to chapter five, which discussed the relationship between social science and common-sense with respect to accounts of non-response. This chapter differed from the other three in that there was no initial foil of explanatory theorising followed by an alternative descriptivist analysis. Instead, social scientific accounts of non-response became the data for analysis. At a general level, it was shown that these accounts were dependent upon everyday practical reasoning and background knowledge - in short, common-sense. More specifically, it was argued that central to the seamless intelligibility of male infertility as a problem, was the 'natural' use of the documentary method of interpretation: social scientific accounts of non-response (particulars) were intelligible through utilisation of existing background knowledge of infertility (underlying pattern), and each was used to elaborate the other. It was argued that through the

complex interconnections between social science and common-sense, an understanding of male infertility as a problem was reproduced. Nowhere did I address the question of whether, when, in what ways, or for whom, male infertility is a problem. In this respect the analysis was 'indifferent', that is, no sides were taken on the question of whether male infertility 'really' is a problem or a sensitive issue.

However, alongside this indifferent approach, the chapter also displayed a critical element, again different from the other empirical chapters. Previously, criticism had been addressed towards explanatory sociological theories with the point made that these paid insufficient attention to endogenous structures of interaction. But in chapter five, the criticism centred around the broader question of how social scientists should speak about the subjects of their research. A case study was provided which showed how male non-response and unexpected accounts of respondents were analysed using ironic methods, in particular a 'politics of experience'. The experience of male infertility was constituted via an Other: the accounts of wives, researchers, and generalised others (what Everyone knows), were privileged over the accounts and non-accounts of infertile men. The critical stand was taken that this was definitely not a desirable research practice.

Clearly, what researchers say about the phenomena they study can be incorporated, in myriad ways, in everyday practical action. Ethnomethodology sensitises us to the possibility that while substantive claims are open to critical rebuttal - for example, the claim that infertility is a threat to male virility is debatable by

members - in the very focus on substantive claims, the question of how phenomena are available in the first place is glossed over. Part of McHoul's argument is that members (and analysts) do have the ability to question the limits and bases of their actions. However, as Pollner's seminal work on reality disjunctures (1975, 1987) has shown, it is usually only when an irresolvable disagreement occurs that argument moves from debate about what the world is like, to debate about the way the world is perceived; in the latter instance, one party or both argues that the other's mode of perception itself is faulty. While there was some ambiguity about the effects of infertility on men, it was far from a state of irresolvable disagreement; in fact there was more of an overwhelming taken-for-granted assumption that male infertility is a problem. Hence, there appeared to be little consideration of the question of how the problem of male infertility might be available for description in the first place.

In the absence of such consideration, analysts utilised ironic methods in their accounting work, and while criticism of this tendency pervaded the study, it was in chapter five that we saw most clearly how ethnomethodological critique of ironic methods can have socio-political implications. The crucial point to be made here is the simple one that form and content are intimately connected. If we gloss content as the claim that male infertility is a problem, and form as partly the process of its discovery by ironic methods, then what I tried to show was how ironic methods produced both a privileged and conservative version of the experience of male infertility. It was privileged in the sense that researchers deigned to speak for non-respondents and to manipulate the accounts of those who did respond.

It was conservative in the sense that it came from within a powerful institution (academic research), and promulgated the interests of that institution (reproducing 'problems' to be researched). Further, in reproducing the understanding that male infertility is a problem, the analysts' accounts contributed to the stifling of an alternative view: that male infertility may not be a problem. That is, not essentially 'not a problem', but 'not a problem' in various local situations with correlative socially organised methods for finding male infertility as 'not a problem'.

While the relationship between form and content is inescapable, researchers can attempt to avoid the use of overly ironic methods. In my view, the standard ethnomethodological inquiry produced in this study -describing members' methods of disclosing, describing, laughing about, and, in general, making intelligible, aspects of male infertility - is a good way to avoid excessive irony. Further, to argue for this approach and against the use of ironic methods is a minimal form of socio-political critique. But a recognition of this critical aspect of the study requires some reconceptualisation of 'political'. McHoul's arguments are an important part of such reconceptualisation: in one of his first attempts at expressing the ideas found in the debate with Bogen and Lynch he commented that,

The way they [ethnomethodological studies] make their contribution might perhaps be this: instead of having the social scientist reproduce a model of the social actor as either cultural or political dope and instead of producing difficult syntheses ... where privileged individuals supposedly know intuitively the real conditions of their social existence, we can show that the political is through-and-through a matter of everyday practice - and vice versa. And instead of a relatively purist concern with

the structure and 'forms' of everyday conduct, irrespective of their supposed 'content' (political or otherwise), we can show as Harvey Sacks suspected that these 'content' considerations are just as much matters of technique ... That the political turns out to be technical doesn't mean that the politics, of, for example, sign following gets subsumed; it simply points to the manner of its investigation. (1984: 76)

Clearly, the difficult task is to link up suitably intricate investigation of the technical with practically relevant socio-political critique. The difficulty of this task should not be underestimated, and in many ways the difficulty itself can only be realised through actually attempting the linkage. The closest this study has come to such a linkage is the analysis of accounts of non-response; unfortunately I cannot claim to have brought out the politics of male infertility through consideration of its technical aspects consistently throughout this study. Nevertheless, I have avoided promulgating either, a "cultural dope" type account of male infertility, or a "privileged individuals" account; and more importantly, I have made a start at intricate investigation with the realisation that there is the possibility of linking such ethnomethodological analysis to critical comment. This is an important first step.

Overall, in terms of existing sociological studies of infertility, this study represents a new approach. Moreover, this original aspect constitutes a minimal form of practical political action in itself. After the 'linguistic turn' in social theory, discursive actions are now conceptualised as the means by which the social is created - our world is made up of socially available means of representation. To return to a point McHoul made in the debate, if no speech or writing is

exempted from this process, then something important can be noted about how social research might make a point and deliver its goods:

To speculate on 'delivery systems' and the like for 'making' emancipatory social science 'arrive' at marginal destinations is to ignore what both Wittgenstein and Garfinkel saw so clearly: to imagine a language is to imagine a form of life. Thus: to speak or write 'anew', even minimally 'anew', is to change, to remake a form of life. (II: 529)

Utilising insights from ethnomethodology to analyse 'male infertility' has been a form of writing 'anew', and through it this study makes a practical point and minimally changes a form of life.

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